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THE BEHAVIORISTIC INTERPRETATION OF CONSCIOUSNESS I.

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- I. The Present Forms and Limitations of Behaviorism
- II. The Evidence for a Mind-Body Problem
 - The Distinguishing Features of Consciousness
 - The Subjective Definition of Awareness
 - The Attributes of the Elements of Content
 - The Problem of the Organization of Consciousness
- III. Vitalistic Arguments
- IV. The Behavioristic Solution
 - Restatement of the Problem
 - The Conscious Machine
- V. Non-experiential Arguments
 - The 'Ineffable' Character of Consciousness
 - The 'Two-aspect' Doctrine versus Behaviorism
- VI. Lack of a Subjective Criterion of Consciousness
- VII. Consciousness as Physical Organization
- VIII. Science and Sentimentalism in Psychology
 - The Psychology of Mechanistic and Teleological Systems
 - Valuation versus Scientific Description
- IX. The Behaviorist Program

I. PRESENT FORMS AND LIMITATIONS OF BEHAVIORISM

Behaviorism, beginning as a laboratory technique and a critique of method in concrete experiment, has advanced rapidly to a place as an accredited system of psychology, stressing the importance of objective methods and of physiological interpretations. The history of the movement is still reflected in the tendency of its exponents to stress experimental method rather than interpretation, in the lack of any systematic formulation of the relations of the science to the

specific problems of the older subjective psychology, and in a certain shifting of ground in behavioristic discussions which indicates that the behaviorists themselves are not yet quite certain of the philosophic implications of their system. Too often a statement of an extreme position is followed by a partial retraction or qualification which leaves the reader in doubt as to the degree of heterodoxy expressed. This hesitation before the plunge is not discreditable to the behaviorist: so great a departure from tradition in psychology demands caution. Moreover, the behaviorist is primarily an experimentalist and believes that many of the supposed problems of philosophy will, with increasing knowledge, resolve themselves into concrete laboratory problems. Why then dispute fruitlessly and at length about them before data are at hand for their solution?¹

Nevertheless, preoccupation with experimental problems will not excuse behaviorism for certain apparent inconsistencies in its doctrines. In various discussions of the scope of behaviorism three distinct and incompatible formulations are discernible. All involve the conviction that a complete description and explanation of behavior can be given in terms of the physicochemistry of bodily activity. They differ, however, in the place assigned to 'mind' in the system. The formulations are as follow.

1. Facts of conscious experience exist and are capable of treatment, as distinct from behavior. The behaviorist is not interested in them, since they are irrelevant to his problems, and leaves them to the tender mercies of the introspective psychologists or philosophers. This is merely psychophysical parallelism with emphasis on the physical. It is the view of Bechterew (2) and other early objectivists.

2. Facts of conscious experience exist but are unsuited to any form of scientific treatment. This is the most common formulation of the behaviorist's position. It seems to have been Watson's view in his earlier writings (31), as is shown by the following statement:

¹ Watson (32) has emphasized this view in his discussion of the rôle of the observer in experimentation.

"Will there be left over in psychology a world of pure psychics, to use Yerkes' term? I confess I do not know. The plans which I most favor for psychology lead practically to the ignoring of consciousness in the sense that that term is used by psychologists today. I have virtually denied that this realm of psychics is open to experimental investigation. I don't wish to go further into the problem at present because it leads inevitably over into metaphysics. If you will grant the behaviorist the right to use consciousness in the same way that other natural scientists employ it—that is, without making consciousness a special object of observation—you have granted all that my thesis requires."

Watson seems now to have abandoned this position for the more extreme one outlined below (3). Weiss (33) still holds to the view when he says, "The structuralist point of view can, of course, be consistently maintained. There is justification for inferring the existence of a conscious correlate for at least some of our actions, but the heuristic value of this assumption seems doubtful when it is shown that behaviorism is not less discriminative or descriptive than structural psychology...." And again, "Perhaps the distinguishing difference between the functionalist and the behaviorist lies in the fact that the behaviorist disregards the entity which the functionalist calls consciousness" (34).

This we may call a methodological behaviorism. Experimentally it promises much, for it avoids the confusion of terms and issues inherent in systems which try to treat of both 'mental' and 'physical' data indiscriminately. It limits the problems definitely to interpretation upon one set of premises and avoids the common error of much older psychology in mistaking a psychological name for a physiological explanation. But it puts the behaviorist in the position of the dog in the manger. It omits a whole universe of phenomena, which have been supposed to constitute the chief realm of psychology. Simply because he can make nothing of the facts of consciousness (which he admits are facts) in his system of physical causation, the methodological behaviorist refuses to believe that any other system can be

devised which will permit the development of a science of pure psychics. And so long as he admits the existence of a universe of consciousness he lays open to attack his major premise, that behaviorism can account for all human activities. For the psychophysical dualist is constantly finding mental facts which he holds to be inexplicable in any mechanistic terms and by refusing to discuss such data the behaviorist prohibits himself from answering arguments based upon them. Moreover, so long as he admits the existence of entities in human existence which behaviorism disregards, he can not deny to others the right to try to study those entities and reduce them to a science by any means whatever.

3. The supposedly unique facts of consciousness do not exist. An account of the behavior of the physiological organism leaves no residue of pure psychics. Mind is behavior and nothing else. This view is implied in much of Watson's writing, although it is not stated in so many words. For the most part he expresses a methodological behaviorism, but such statements as the following leave little doubt of his fundamental denial of the fact of consciousness, as described by the subjectivist. "It is a serious misunderstanding of the behavioristic position to say, as Mr. Thompson does—'And of course a behaviorist does not deny that mental states exist. He merely prefers to ignore them.' He 'ignores' them in the same sense that chemistry ignores alchemy, astronomy horoscopy, and psychology telepathy and psychic manifestations. The behaviorist does not concern himself with them because as the stream of his science broadens and deepens such older concepts are sucked under, never to reappear."

This is the extreme behavioristic view. It makes no concessions to dualistic psychology and affirms the continuity in data and method of the physical, biological, and psychological sciences. "Consciousness is behavior." "Consciousness is the particular laryngeal gesture we have come to use to stand for the rest." I shall speak of this doctrine as strict behaviorism, or for brevity simply as behaviorism, since methodological behaviorism is only a form of epiphenomenal-

ism. Such a behaviorism has been called a materialism by several recent critics.¹ Perhaps it is such, to the extent that modern physics and physiology are materialistic, but the word materialism implies a metaphysical theory of reality, whereas these sciences are, at least in their systematic treatment, altogether phenomenological. Psychophysical dualism and epiphenomenalism do imply theories of the ultimate nature of mind and matter, but behaviorism claims to avoid this and to attempt nothing more than a logical and mathematical description of experience such as is presented by the physical sciences. To stigmatize this as materialism is to appeal against behaviorism to the prejudices aroused by a crude metaphysic which is nowhere implied in its doctrines.

When we examine the evidence upon which strict behaviorism is based, a weakness in its current formulations seems evident. The behaviorist denies sensations, images, and all other phenomena which the subjectivist claims to find by introspection or by some form of direct knowledge. This disagreement as to matters of fact is not necessarily fatal to behaviorism, although it is the most frequent ground for rejection of the system. But when we examine the evidence adduced in support of the denial of consciousness, the behaviorist seems to have failed to strike at the root of the dualistic systems. The arguments employed—they can scarcely be called evidence—fall into four chief classes.

(a) Appeals to the principle of parsimony to exclude consciousness because it is unnecessary for the explanation of behavior (32, 34). The inadequacy of this argument is evident. The gravitational effects of Jupiter are irrelevant to a physiology of digestion, yet they are none the less a fact. The behaviorist must show further that his system is adequate to explain the supposed phenomena of consciousness as well as of behavior, before the argument becomes relevant. At best the argument in its present form can support only a methodological behaviorism.

(b) Reviews of the history of evolution or of superstition to show how the belief in the self may have arisen (35).

¹ Cf. Pratt, (33).

This argument is beside the point. It may force the subjectivist to deny the universality of evolution or of historical sequence, but this he has already done in claiming the existence of a universe of non-material things as the subject of his study, and in refusing to admit their derivation from the physical phenomena of evolution.

(c) Redefining of mind in objective terms as when Bawden (1) states that "... mentality or mind is a name for the fact of the control of the environment in the interest of the organism through the interaction of inherited capacities and acquired abilities." Such arguments likewise avoid the issue. They ignore the subjectivist's claim that he knows a unique mode of existence not definable in objective terms. At best from the standpoint of the subjectivist they constitute only a study of the physiological correlates of conscious processes.

(d) Attacks on the method of introspection. These are the behaviorist's big guns. In general, they take one of two forms. Introspection is inaccurate, unverifiable; the stuff revealed by it is variable and inconsistent. It is indeed so unreliable that we are justified in throwing out everything which it claims to establish. But much the same argument has recently been urged against the objective methods of mental testing. It may be a strong plea but it is not logically convincing. A method may be defective and yet reveal fundamentally important facts, as Loewenhoek's observations on infusoria.

The other form involves a theory of introspection. When one examines his own mental states he is really reacting to stimulation of his proprioceptors. Introspection is a physiological process and as such can reveal only other physiological processes. Even if mental states exist, they cannot be discovered by introspection. But the introspectionist may reply, "Your doctrine of introspection is only an hypothesis. You have not produced convincing proof of the homeodetic theory of introspection. My claim that mental states form a unique mode of existence is based on the nature of the material which introspection reveals. If it does reveal such phenomena, it must be more than a physiological process. It is

only by showing that mental states do not exist that you can prove that you have given an adequate account of the introspective process."

Thus it appears that the current formulations of behaviorism have not made good their claim to exclusive possession of the field of psychology. Methodological behaviorism has all the faults of psychophysical parallelism plus that of intolerance. It admits the existence of certain phenomena called conscious, admits that it can not fit them into its system, and denies to others the right to study those phenomena and to seek to formulate them into a science. Or, it reverts to the early objectivism of Bechterew and admits the possibility of a subjective psychology, merely asserting that this psychology is irrelevant to behavioristic explanation. It thus paves the way for the development of two cognate sciences, such as Fernberger (8) has recently advocated.

Strict behaviorism is advanced as a theory, but the insistence upon methodological behaviorism at all costs has prevented the consideration of any supposedly subjective data and has left the theory undeveloped. Yet if behaviorism is to become a complete science, if it is to avoid becoming merely a coördinate system with subjectivism, it must subordinate questions of method, of objectivity, to the application of mechanistic or physiological principles to the whole of psychology. This point is emphasized by Dewey (5) to whom the facts of consciousness appear as an experimental behavioristic problem. "To recognize that the behavioristic principle can make a place for them (the facts of consciousness) is important. For science is, after all carried on by men, and a seeming denial that such facts do exist and do come under the behavioristic principle is sure to keep alive in the minds of some a futile introspectionist method, by setting to one side a realm of facts to which (so it is thought) it *must* be applied since the behavioristic method confessedly does not apply."

Let me cast off the lion's skin. My quarrel with behaviorism is not that it has gone too far, but that it has hesitated, that it has been diverted by details of experimental method,

when more fundamental issues are at stake; that it has failed to develop its premises to their logical conclusion. To me the essence of behaviorism is the belief that the study of man will reveal nothing except what is adequately describable in the concepts of mechanics and chemistry, and this far outweighs the question of the method by which the study is conducted. I believe that it is possible to construct a physiological psychology which will meet the dualist on his own ground, will accept the data which he advances and show that those data can be embodied in a mechanistic system. A behaviorism will thus develop which will be an adequate substitute for the older psychology. Its physiological account of behavior will also be a complete and adequate account of all the phenomena of consciousness. It will be methodological only in insisting that the concepts of the physical sciences are the only ones which can serve as the basis for a science, and in demanding that all psychological data, however obtained, shall be subjected to physical or physiological interpretation.

Such a program demands that we face the issue squarely. We must accept tentatively the supposed data of introspection and test the validity of our system by its ability to deal with such data. We shall find but three alternatives: the data may be of such a character that we can not hope to embody them in any mechanistic system; they may even now fall into such a system; or we may be able to define the problem of consciousness as an experimental problem, unanswerable on the basis of existing data, but offering possibility of solution with the development of objective science. Indirect arguments and denials of facts which others consider verifiable will not suffice. The dualist advances specific data with certain definable attributes as evidence for the validity of his system. Perhaps we can not verify his findings objectively, but we can examine his claims and determine if and in what respects they are incompatible with the postulates of the natural sciences. The key to the development of behaviorism lies here. When the behaviorist denies that consciousness exists, he denies, not the existence of the

phenomena upon which the conception is based, but only the inference that these data constitute a unique mode of existence or that they are not amenable to analysis and description of the same sort as are 'physical' data. Unfortunately, the psychological terminology current today involves not only an enumeration of phenomena but also a definite theory of reality. It is this theory which behaviorism repudiates.

In the following pages I shall first seek to discover on the basis of introspective evidence the 'data of consciousness,' stripped of metaphysical theory. I shall then attempt to show that these data are adequately describable in the concepts of the physical sciences and that the addition of a dualistic interpretation adds nothing to our understanding of them.

II. THE EVIDENCE FOR A MIND-BODY PROBLEM

Before we can begin a constructive program we must define clearly the sort of data with which the behaviorist and the dualist claim to deal and must have in mind the presuppositions underlying the system of each. I am not concerned here with the development of an epistemological theory, but only with the empirical basis of the distinction between behaviorism and psychophysical dualism. A system of psychology can not be developed without some implied theory of knowledge, but, on the other hand, every theory of knowledge presupposes a theory of psychology and by changing the rules of the game innumerable self-consistent systems can be developed. Escape from this dilemma, in so far as it concerns the points of difference between dualistic and behavioristic psychology, is offered by the faith which both express in the validity of physical science. Both behaviorism and psychophysical dualism accept the formulations of physics, chemistry, and biology as adequate descriptions of the interrelations between certain data of knowledge, and hence both accept a theory of knowledge which must justify the methods and conclusions of physical science. Any theory of knowledge which does this must also permit an attempt to extend the methods of physical science to other aspects

or elements of experience and can not arbitrarily limit the field of physical investigation. The dualistic systems of psychology admit this and seek to justify their dualism upon empirical rather than epistemological grounds. They point to certain data of experience—qualitative diversity, transcendence of time and space, independence of ‘physical law,’ and the like—and assert that the concepts of the physical and biological sciences are inadequate to describe the relations and characteristics of these data. On this ground they justify the division of experience into two aspects or modes of existence and the formulation of additional postulates concerning the nature of ‘mental existents.’ The behaviorist, on the contrary, claims that the concepts of the physical and biological sciences are adequate to describe and account for the whole of experience and that there is not adequate empirical evidence for the distinction of mental and physical modes of existence or aspects of experience. “Grant me,” he says, “the postulates of the physical sciences, and I can show you how the phenomena of mind may arise within a system which has no other attributes than those which the physicist ascribes to his phenomenological world.”

The mind-body problem is thus a problem of the applicability of certain postulates and descriptive methods (those of the physical sciences) to certain specific data of knowledge (the so-called attributes or elements of consciousness). The controversy between behaviorism and dualism is not a question for philosophy but one to be answered strictly in the light of empirical evidence provided by psychological study.

We must first consider the character of the postulates and methods of physical sciences. These sciences are as yet incomplete and no one can predict what form they will finally take. Their simplest formulations at present involve postulates of the relation of discrete entities in time and space and the attempt at characterization of experience in terms of the mathematical relations of these entities. They seek to keep their postulates as few and simple as possible; to avoid ascribing to the entities other attributes than those implied in a time-space-number system; to avoid additional concep-

tions of energy, substance, and the like.¹ Complete success has not attended their efforts at simplification, but the physical and biological sciences have found it possible to develop with but few additions to the above named postulates. In general they have tended to quantitative formulations, with their implications of individual discreteness and qualitative identity of elements. I shall not attempt here to characterize their methods further than this, since the later attempt to deal with the phenomena of consciousness in physiological terms will give additional definition to the method.

The conception of mind has undergone a long course of evolution and many of its supposed attributes are only vestiges of the superstition, religious dogma, and false psychologizing which at various times have influenced its progress. Of these, many do not fit into the physical system, but we shall find that they are the illusions of a metaphysical legerdemain and not the discoveries of introspective psychology. Before we can attempt a behavioristic account of consciousness we must scrutinize these attributes and discard such as do not seem to be revealed by psychological investigation. Then we may begin the application of the methods and postulates of the physical sciences to the residual data.

The Distinguishing Features of Consciousness

There are almost as many analyses of conscious phenomena as there are writers on the subject and from the mass of frequently vague and conflicting discussion it is difficult to distinguish just what characteristics are held to differentiate conscious phenomena from the subject matter of the inorganic sciences. The following, however, seem to be the most frequently stressed and the ones upon which most general agreement may be obtained.²

¹This mode of attack is not fashionable in philosophy today. Realism believes that it has scotched solipsism. But a consistent behaviorism can not admit any accurate direct knowledge of reality, since, if reactions constitute knowledge, the reactions may be to a part only of the total situation and knowledge is, therefore, limited by the reaction capacities of the mechanism. The behaviorist is under no delusion as to the 'ultimate' truth of the physical system. For him it is only an explanatory hypothesis, accepted because it seems the most flexible and widely applicable of all which have been suggested.

²I believe that the chief difficulties of the mind-body problem arise from such

1. *Awareness.*—The conscious organism has a knowledge of itself, of things other than itself, or of both which the inorganic mechanism, however complicated, lacks. Awareness may or may not presuppose a knower; it presupposes something known. It does not imply any particular pattern or organization of the known. It may or may not presuppose the doctrine of transcendence discussed below.

2. *Content.*—This is a universe of things known, of sensations, images, affects, etc., which stand in the relation of objects of awareness and which have certain attributes not definable in spatial, temporal, quantitative, or other 'material' terms.

Various writers have stressed these categories in different ways. For one, the knowing is the important thing and content is merely attribute of knowing. For another, content alone exists and when its characteristics are described, nothing need be said of any process of knowing. Content is sometimes physical reality distorted by the process of knowing, sometimes distinct from physical reality, a parallel mode of existence. That is, red may be ether vibration as known, or the psychical correspondent of ether vibration. But whether we are confronted with a pink awareness or an awareness of pink, the attributes of process or content which distinguish it from the physical world seem to be very much the same. For brevity of discussion I shall ascribe them to content and later discuss conditions where they seem rather ascribable to awareness.

Things known, then, have certain attributes which are held to mark them off as unique from a physical reality. The more important of these are:

(a) *Qualitative Diversity.*—Sensations, images, affects, have certain attributes—duration, intensity, extensity, quality, clearness, and the like. Of these duration, intensity, extensity have their parallel in the physical world and are not peculiar analysis as this with the failure to appreciate the fact that the 'elements' are abstractions whose existence is conditioned by the intactness of the total organization of consciousness. We may speak of an element of consciousness but not of a conscious element. Confusion on this point has led to the various atomistic theories and to much meaningless discussion of consciousness in lower organisms. (See page 000.)

to consciousness, but quality and clearness (which is often reduced to a different kind of quality), form a unique existence. Differences of quality are not implicit in physical postulates and are not describable in mathematical terms.

(b) *Self-transcendence*.—The content of consciousness (or the conscious process) transcends time, space, and objective discreteness. The material in content unites past, present, and future, relates spatially separated objects in a unique unity, includes not only the explicitly known but also implicit meanings. This is sometimes stated as a function of awareness sometimes as an attribute of content, sometimes as the very essence of consciousness.

Sometimes content is held to transcend physical reality, as when an image refers to the past. Sometimes awareness is said to transcend the elements of content, as when two images are known together and compared. The problem of transcendence seems to be essentially the same in either case. It is the basis of the claim for psychological uniqueness in memory, recognition, meaning, purpose, and the unity of consciousness. Even the problem of qualitative differences has recently been reduced to a peculiar union of discrete neural impulses.

3. *The Organization of Consciousness*.—In addition to the processes or elements making up awareness and content, we may distinguish certain characteristics which may be ascribed to the organization of things known into the complex system of human consciousness. They are:

(a) *The Limitations of Content*.—In the field of consciousness certain elements are included, others excluded. This selective action is sometimes cited as having no parallel in the material world.

(b) *The Unity of Consciousness*.—This is perhaps implied in the doctrine of self-transcendence. The elements of content are said to be fused into a unique whole which is something more than mere coexistence. Knowledge of the elements transcends the elements. The 'centrality' of consciousness is unique from the physical world.

(c) *Consciousness of Self*.—Through the warp of conscious-

ness there runs a thread of self-knowledge. This is not necessarily a knowledge of the knower, but is a feeling of personal identity which is a part of content and is distinct from other parts.

(d) *Self-arrangement*.—Under this heading I mean to include the various capacities of logical necessity, self analysis, intelligent action, and the like. These may be generalized as the capacity of the elements of consciousness to fall into ordered patterns, or as the ability of consciousness to define order within itself. Here we are treading upon dangerous ground, for to question the basis of logical analysis is to become involved in a scepticism which throws doubt even upon its own doubting. Nevertheless some of these capacities are held to distinguish the organization of consciousness from physical order and hence must be considered in a discussion of the behaviorist's problem. In the light of its premises, behaviorism must study the logician and discover how his logic arises from the interaction of propagated disturbances in his nervous system; it must study the scientist and show the material basis of human progress; it must study the moralist and discover the mechanism of his ethics.

This classification is not complete, but I believe that the more important arguments for the uniqueness of consciousness will fall into one or another of the categories listed. There is little unity or similarity among the affirmed elements and attributes of consciousness save the supposed impossibility of describing them adequately in terms of the concepts of physical science. If we can include those above in our behavioristic system, there will be little left upon which the subjectivist may base his claim to a distinct system of knowledge.

I shall now take up the questions raised by the dualist in greater detail, examine the subjective or introspective evidence which is supposed to prove that the various attributes of consciousness are different from the phenomena of the physical world, and try to show that the subjective evidence does not justify the demand for any other postulates than those made by the inorganic sciences.¹

¹ Watson has repeatedly suggested that in the physical sciences the question of the observer is presupposed and disregarded and that behaviorism may follow the

The Subjective Definition of Awareness

Of introspective description of the process or state of knowing there is none, although many pseudodescriptions have been advanced. The neo-realists have given us a statement of the case for awareness which none of the other schools has been able to refute. I shall follow them, with some obvious deviations, in the subsequent discussion.

There is no direct experience of a knower. There is no direct knowledge of the process of awareness. All that can be discovered by the most careful introspection is the existence and attributes of the objects of knowledge, of the content of consciousness, and this content does not include the knower or awareness itself. Knower and knowing are implicit in the known, but are not directly experienced. That something produces the limits and attributes of content is a logical conclusion, but no description of that thing from experience is possible. All that can be said is that some process, relation, or what not, gives rise to the phenomena of content, and determines the character of the field of consciousness. Subjective experience does not justify any further statement concerning awareness than this.¹

It follows that any process or relation which will account for the selection of the elements of content and for the attributes of those elements (other than being known), whether that process or relation be in a universe of physical things or same method. I do not wish my position to be confused with this. It is only the postulated characteristics of physical reality in the absence of an observer that I consider here. The mechanism of the observer seems to me a real problem for the behaviorist, as it is not for the physicist. Nor does Watson altogether disregard the problem of the observer. The behaviorist may study a behaviorist in the act of studying a behaviorist, and is justified in concluding that his own processes of study resemble those of the other.

¹ Some writers have read other characteristics into awareness, as does Montague (22) when he defines consciousness as ". . . the potential or implicative presence of a thing at a space or time in which that thing is not actually present." But such statements are mere inferences from the nature of content. Because the thing known has certain attributes it is assumed that the knowing process has those attributes. This assumption is perhaps justifiable, but the point which I wish to emphasize is that, if the characteristics are not found in content, there is no other reason for ascribing them to awareness. If content does not transcend time and space, then neither does awareness. The only criterion of the process is the result.

in a realm of pure psychics, will fulfill all the subjectively discoverable requirements for a complete account of awareness. The subjectivist can not deny that any process whatever which will give rise to the characteristics of the known is the process of knowing. It is unnecessary, therefore, for the behaviorist to deal specifically with awareness. If he can give an account of the attributes of content, his task is accomplished.

The Problem of the Attributes of the Elements of Content

The two characteristics of the elements of content which are held to differentiate them from the data of physics are their peculiar quality or qualities and their self-transcendence. The psychological account of quality, as of awareness, is almost wholly negative. Quality is something unique, indescribable, except in terms of itself. Red is red, green is green. Neither is, by any stretch of imagination, a form of ether vibration or chemical change in the brain. This, of course, is crude subjectivism. Modern philosophy is more subtle. Quality is a fusion of discrete elements into a unique whole: it is the process of fusion, not the result (26). But the fact of qualitative diversity remains the basis for the argument. The fusion is deduced from the uniqueness of quality, not from any direct knowledge of the process. The concept of transcendence has been here introduced upon no other grounds than the existence of quality.

Let us examine the situation more closely. What has the subjectivist to say in description of quality? Qualities are diverse; some are less unlike than others; not all seem simple but those which are compound are compounded of simpler qualities, and when by analysis the simplest qualities are reached, nothing more may be said of them save that they are in different, undefinable degrees diverse.¹ They have no describable characters inherent in themselves; they are not analyzable into anything else. They exist by virtue of their

¹ Holt (13) has advanced evidence to show that all qualities are subjectively analyzable, his implication being that if introspection could carry the analysis far enough quality would reduce to some neutral, non-qualitative substratum. This is also the thesis of my discussion, save that the neural mechanism of introspection later developed seems to preclude possibility of any such ultimate subjective analysis.

undescribable differences and by virtue of nothing else discoverable by introspection.

For the subjectivist this is not the crux of the matter. He holds that quality is something apart from unanalyzable diversity, a thing-in-itself; red would always be recognizably red, though there were no other quality from which it differed. My point, however, is that the subjectivist can tell nothing of the process by which he knows quality-in-itself. He can neither affirm nor deny on introspective grounds that mere unanalyzable diversity is the source of this appearance of quality-in-itself. Therefore, the behaviorist is fully justified in assuming unanalyzable quantitative diversity as the sole condition of quality, provided that he can thereby show how the appearance of quality may arise and that he violate no requirement for description of other attributes of content.

On the basis of his own evidence the psychophysical dualist is compelled to define quality as a diversity which is not analyzable by the process of awareness or introspection. He can not, on introspective grounds, define the process of introspection. He can not otherwise define quality. It is merely something which is refractory to subdivision (analysis) by something else. But this is nothing unique from the physical world. If the behaviorist can show any system which is unitary in its relation to any other system in the behavior of the organism, which is therefore unanalyzable by that system, he will have met all the subjective requirements for an explanation of qualitative diversity and 'quality-in-itself.'

The doctrine of the image has occupied a rather large place in discussions of behaviorism. The existence of 'centrally aroused sensations' has seemed to offer considerable difficulty for a methodological behaviorism, since such sensations are presumably not open to objective study. For a behaviorism which is chiefly interested in physiological explanation, the difficulty is less serious, since it makes little difference in physiological principle whether a neural pattern is aroused peripherally or centrally. Nevertheless, on empirical grounds I am inclined to agree with Watson's reduction of the

image to terms of reaction. The sharp issue on matter of fact which that interpretation has induced seems, however, to call for some modification of the original formulation. The majority of psychologists claim to find peripheral sensory elements in their images. In my own, I find the condition to be as follows. The visual image is made up largely of the feel of movement, with a core of true visual quality. This, on closer examination, turns out to be an actual entoptic stimulus-retinal light or after-image—which is interpreted in terms of the motor activity. Thus an entoptic light, aroused by pressure, was successively interpreted as a human face, a wolf's head, and the wing of a flying bird, in accord with changes of the motor set.

Recent developments of the 'Gestalt' or integration theory suggest that the attributes of sensation are likewise dependent upon the reactions of the observer.¹ Sensory quality, intensity, movement, and extensity vary with the condition of the observer. All sensations are hence regarded as perceptions and 'pure sensation' becomes a meaningless abstraction. This conception, with the above view of the image, would make a continuous series of sensation, perception, after-images, memory images, illusions of day-dreaming, hypnagogic images, dream images, and hallucinations; the quality, vividness, and seeming reality of the experience varying with the character and degree of dominance of the interpretative set. Such part of the introspective literature on the image as does not obviously suffer from the stimulus error seems to bear out this view. Images are fleeting things and where the seeming peripheral sensory elements are actually described they have more the character of entoptic lights than of detailed pictures.

Whether or not this account of the image is correct, the image seems to present practically the same problem for behavioristic interpretation as does sensation. It contains qualitative elements which are not describable. In addition it is supposed to contain reference to the past, future, or to some spatially distant object, and hence to transcend itself or space or time.

¹ Cf. Koffka (15).

This doctrine of transcendence is today by far the more fashionable argument for the uniqueness of consciousness. This is true, partly because the subjectivists themselves have so nearly discredited subjective quality. Perhaps it is true, also, because there has been so little careful psychological study of this supposed characteristic. Whatever the cause, it is apparent that the doctrine of self-transcendence of mind is today dominant in discussions of psychological theory. It takes form in discussions of recognition, memory, purpose, spatial reference, and meaning. I quote statements here which represent extreme views of the psychic transcendence of time and space.

"Suppose we remember a visit to the Azores 20 years ago. That original visit, we are told, left paths in the nervous system, along which resistance is diminished, and the nervous discharge tends to follow those paths. But this physical account misses the essence of memory. The neural event is a present fact, similar to one that happened in the nervous system before, but not in any sense that past event; while in memory the past event is present. There is here a direct incompatibility between memory and the laws of material existence. Materially the past event is quite non-existent; mentally it is not, for it is present (with all its pastness too) as a part of our conscious experience. No matter whether it is directly present as if in a sort of perception, or present only as something not seen but meant or inferred. In either case it is an object touched by present consciousness; for inferring is a conscious act. Nor does it matter whether we say that the past event is relived in the present, or the mind leaps back into the past. In either way the gulf of time is bridged. But physically this sort of thing cannot happen, for a present physical event can not be or contain or touch an event that happened 20 years ago" (26).

"The organism is separated by space from the object to which it responds; mind with infinite speed passes from one to the other" (26).

"Thought constantly deals with the distant in space and with the remote in time; but the movements of the 'language-

mechanisms' in which the thought of a given moment is supposed to consist are strictly intracorporeal and are limited to that moment" (18).

There is not space here to review the arguments for transcendence in detail. The statements usually take one of the following forms:

1. Content transcends physical time and space
 - (a) By reproducing or invading the past or the spatially distant and bringing it, representative or real, into the present.
 - (b) By making physical diversity into unity, as in sensation.
 - (c) By referring or pointing to past or future, without actually bringing them into content.
2. Content transcends itself
 - (a) By identifying present content with past or future content.
 - (b) By uniting its own elements into a whole whose parts may be compared, yet form a unique unity. (This is also expressed in the doctrine that awareness transcends the elements of content.)

The first doctrine holds to an objective reality which is transcended by the non-objective. But this demands an explanation of falsification of memory, and the like, which has not been provided. I may imagine a remotely past object which once existed (*a*); I may imagine a past object which never had physical existence (*b*). How do these images differ? Both have past 'reference,' both 'point back.' I can determine that one refers to a physical past only by the correspondence of present physical evidence with present content. I conclude that (*a*) refers to a real event because of 'historical proof.' I deny it in the case of (*b*) because of lack of similar proof.

The same is true for events within my own memory. I remember that I locked my door. I later find that I did not. Only by correspondence of present physical evidence with content of memory can I establish that an objectively past event is or is not present in consciousness. The same may

be said of reference to a spatially remote object or of realization of purpose. The actual reference to a physical object can be established only by other physical evidence of that object. In this respect, a photograph is as much a slice of the past as is my memory. The reference is independent of the physical existence of the object. It is either a purely subjective feeling of pastness, or it is an inference drawn from the correspondence of present content with present physical evidence of former or distant events.

If we adopt a purely subjective view, the same argument applies. I remember that I remembered the incident of locking the door. Does this refer to an actual past content of consciousness, or is it but another falsification of 'memory'? 'Introspectively,' I can not determine, but I find above, evidence on the written page that I did so remember. A present content having 'sensory reference' corresponds to another content having 'past reference.'

The past state of consciousness is not recalled into consciousness, but another appears, containing the feeling of 'pastness.' The identification of this content with the past content, implied by the doctrine of transcendence, is the result of a false inference from some objective evidence or from some correspondence of 'memory content' with 'sensory' content. Thus we see that the supposed pointing of content is nothing more than a subjective feeling of pastness, remoteness, or futurity, which is unrelated to the real existence of the past, remote, or future event or object.

What is the nature of these feelings of pointing or reference? The introspective literature deals extensively with them. The introspectionists who seek to describe the objects of consciousness fall into two chief classes, the structuralists and the exponents of imageless-thought. The latter include in the objects of knowledge sensory content and process. In many cases the processes are, in the words of the observers themselves, merely inferences from the sequences of content. 'Judgments—problems and solutions—must be conceived as something more than successions of images. The latter will not account for the results attained. The results are evidence

for the existence of something more than the images.' But in other cases direct experience of process is claimed. I confess that I find these discussions almost unintelligible. The processes are awareness of meanings, fringes of content, irradiations, placid convictions, directions of thought, indescribable qualities of familiarity, *Bewusstseinslagen*. They seemingly have no other attributes than that of pointing, or implying.

As one reads the descriptions it seems as though the authors were trying to describe vague feelings: their words, as Titchener (27) says, have an 'emotive ring.' As we have seen, they are independent of real existents. They point to nothing present in consciousness, they point to nothing outside of consciousness. They are directions with nothing at either end. But is not such pointing from nothing to nothing sheer nonsense?

At this moment comes a call to lunch. I am reluctant to go. I have the feeling of swelling potentiality, of unexpressed volumes ready to pour from my pen, a magnificent *Bewusstseinslage*! But it is nothing more than a tenseness, shallow breathing, muscle tonus, enteric stagnancy, which remains unmoved by the suggestion of food. It points to nothing. It does not tell me what I shall accomplish. It is indistinguishable subjectively from the enthusiasm aroused by a progressing experiment. If I stop to introspect, it leads to the verbal expressions of 'swelling potentiality, etc.'—to this discussion. If I do not introspect, it merely keeps me at work, without other meaning until it is succeeded by another content. It means nothing in itself. Only as it leads to verbal expression or to accomplishment does it acquire meaning.

As a behaviorist I am disqualified for introspection. But there is authoritative introspective evidence in support of my contention. Titchener (27) has dealt at length with meanings and our transcendentalist friends will profit by re-reading him.

"I hold that, from the psychological or existential point of view, meaning—so far as it finds representation in consciousness at all—is always context. An idea means another idea,

is psychologically the meaning of that other idea, if it is that idea's context. And I understand by context simply the mental process or complex of mental processes which accrues to the original idea through the situation in which the organism finds itself—primitively the natural situation; later, either the natural or the mental. In another connection I have argued that the earliest form of attention is a definitely determined reaction, sensory and motor both, upon some dominant stimulus; and that as mind developed, and image presently supervened upon sensation, this gross total response was differentiated into three typical attitudes; the receptive, the elaborative, and the executive, which we may illustrate by sensible discrimination, reflective thought, and voluntary action. Now it seems to me that meaning, context, has extended in the same way. Meaning is, originally, kinaesthesia; the organism faces the situation by some bodily attitude and the characteristic sensations which the attitude involves give meaning to the process that stands at the conscious focus, are psychologically the meaning of that process. Afterwards, when differentiation has taken place, context may be mainly a matter of sensations of the special senses, or of images, or of kinaesthetic and other organic sensations, as the situation demands. The particular form that meaning assumes is then a question to be answered by descriptive psychology."

In other words, the only way in which an element of content may have meaning is by coexisting with or by leading to another element of content, which is then the meaning of the first. Here is no mystic transcendence of time or space, no pointing from naught to naught, no fullness of meaning of nothing.

The fact is that meaning, on subjective analysis, reduces to a succession of images accompanied by vague affects, and to ascribe transcendence to it is to mistake logical inference for introspective analysis.¹ The behaviorist need only account

¹ I can not agree with Sheldon (26) that such inference involves the problem of transcendence. For psychology it is only the generalization of certain types of experience. Pastness, for example, is a name for a class of experiences having cer-

for the determination of the succession and for the quality of the affect.

There remain, of the transcendence hypotheses, the transcendence of physical discreteness in sensory quality and of the discreteness of the elements of content by consciousness. How do successions of ether vibrations or neural impulses become unitary in sensory quality? How may two elements of content be known together and compared in consciousness? The answer to the two problems is the same. On the one hand there is a system of elements which are by definition disparate. On the other, a union of these elements in the relation of being known as one. Introspection can tell nothing of the process by which this unity is brought about. The process can be defined only in terms of its products, quality and the 'conscious manipulation' of content. The keys are united by the ring. This union differs from subjective unity solely in that the keys do not thereby acquire quality or the capacity for self-ordering. Subjective unity in itself presents no problem.

I have devoted so much space to the doctrine of transcendence because its rejection seems to me essential to the progress of psychological science. Its acceptance disregards the empirical findings of both the introspectionists and behaviorists, leads to the mystic's substitution of emotional for rational conceptions, and abandons the use of scientific method in this field of psychological analysis. The behaviorist is justified in rejecting it as an inference from inadequate evidence, and can cite good introspective authority in support of his view.¹

tain characters (perhaps a specific affect and lack of tension or of demand for immediate reaction) and the inference of pastness is only the assignment of an experience to this category. The inference of *real* temporal relationship is, psychologically, the translation of succession into a spatial or numerical series which can be thought in postural terms.

¹ The new realists have met this problem by pointing out that our conceptions of the character of time and space are only postulates and that if mind seems to transcend time and space, the fault lies really in a false notion of the latter. In real time or space objects are related as they are in mind. Modern physics has done much to revise our ideas of space-time relations but has not added the postulate of self-transcendence. And the evidence from mind does not seem to me to justify the addition. In this respect neo-realism seems to me a form of animism.

Certain problems of 'reference' remain, but they are experimental, not philosophical problems. "The particular form that meaning assumes is then a question to be answered by descriptive psychology" says Titchener (27). "It [the problem of meaning] becomes like others in psychology a problem for systematic observation and experimentation," says Watson (32). The behaviorist must describe the particular patterns of proprioceptive reactions which lead to the statement, 'That occurred long ago,' he must define the conditions of response which constitute recognition, and the like, but he need not seek a mystical self-transcendence in the physical world when none exists in the so-called psychic.

The Problem of the Organization of Consciousness

I have thus far dealt with the elements of conscious content, which, occurring in various combinations, make up the complex organization which we call consciousness. The tendency among writers of the subjectivist schools is to consider these as capable, at least theoretically, of independent existence, as though there might be awareness of a simple sensation, without other concomitant elements of content, or as though there might be awareness for one moment without preceding or succeeding moments. I believe that the greatest difficulties of the mind-body problem have arisen as a result of the fallacy which is involved in such an analysis. A single element is never experienced in isolation; it is an analytical convenience, nothing more. On subjective evidence one can not assert that a single element can ever be known alone. Indeed one must say that a single element never *is* known except in combination with others. The essence of consciousness is a field of many elements, organized after the plan of human experience. In the discussion of the elements of content I have sought to show that their 'peculiarly psychic' attributes of quality and reference are not intrinsic to them as self-existent elements, but can be defined only in terms of their relationship within the complex organization of which they are independent variables. We must now examine this organization in greater detail to discover in how far it con-

forms with the types of organization discovered by the physical and biological sciences within their realms of investigation.

At any moment the 'pattern' of consciousness consists of a number of elements coexisting in the relation of being known together. The pattern is in a constant flux, new elements appearing and others dropping out with a certain regularity and consistency which provide the basis for the conceptions of logical necessity and physical continuity.

Various dualistic systems have emphasized different characteristics of this organization as evidence for the mind-body problem. The chief arguments from organization are based upon (1) the unity of consciousness, (2) the limitation of consciousness to a part of existence, (3) the persistence of the elements of self-consciousness, (4) the capacity for self-ordering or analysis, (5) the creative activity of mind.

The problem of the unity of consciousness and of the limits of consciousness is essentially that which I have discussed as the self-transcendence of the elements of content. Every system of dualistic psychology has postulated the existence of entities not present in content (indeed, the concept of the unity of consciousness implies the existence of other entities excluded from that unity) with, in brief, the attributes of physical existence. Within this physical system unity is defined as organization in a system whose parts are more closely or complexly related to each other in behavior than to the elements of other systems (for example, a solar system or a physiological organism). This is also a definition of the unity of consciousness. Conscious unity differs from physical unity only in that the elements of the physical system are mathematical entities, the elements of the conscious system are qualitative elements. The argument from unity therefore reduces to the argument from qualitative diversity.

The behaviorist has been strictured for his inability to determine objectively whether a process is or is not conscious, although he admits that some processes are and others are not conscious (19). What determines the content of consciousness at any given moment? A pure subjectivism, involving psychic determinism, may assert that preceding sensations

or images determine subsequent ones and hence the elements of content. But it is unable to explain for example how a momentary redness can determine a subsequent crashing noise. The postulate of a physical world tides over the gap (7). Every system of psychology which has sought to be more than purely descriptive has been forced to fall back upon the postulate of physiological processes to account for the inclusion of specific elements of content. One hears a noise because the ear is stimulated, thinks of the past because he sees something that reminds him of it. There is no subjective evidence as to what determines the content included at a given moment. Introspection may show perhaps that one complex of physiological processes involves consciousness, another does not. If the behaviorist can show a constant difference between these physiological processes he will have fulfilled the subjective requirements for an explanation of the limits of consciousness. Further, as I hope to show when I take up the constructive program of this paper, he need not appeal to introspection to determine whether or not he is dealing with a 'conscious' complex. The 'conscious' will be given in the organization of the complex itself. The limits of consciousness are the limits of an undefinable togetherness. Any togetherness which fulfills the other criteria of consciousness will satisfy the subjectively definable criteria of limitation of content.

The field is sometimes held to be united or given its character of 'centrality' by the consciousness of self, which runs through it. I need only refer to James' (14) description of the self to show that it presents no other problems than those of persistence of sensory elements and recognition. On introspection, the self resolves into a group of sensations, largely somæsthetic, which recur from time to time and, if they are dominant, lead to some internal or explicit expression such as "This is I," which becomes their meaning of self. Associated with this there may be a constant emotional tone, but subjectively, nothing more is discoverable than a constant affective and sensory element associated with ideas of self, which in turn resolve into verbal or imaginal expressions of, "This is I or mine."

The momentary aspects of content can not be separated from the temporal aspects, for the flux of content is continuous, although isolated elements may seemingly persist unchanged while others change. Here the field presents sequences which are classified roughly in accord with the regularity of their recurrence. Certain sequences are so regular as to be taken for granted as though they required no postulated relations to link up their elements. These constitute logical and mathematical necessity. Psychologically they reduce to unvarying sequences of ideas which, in turn, resolve into sequences of sensory or imaginal elements, subject to the same analysis and demanding the same sort of explanation as other sequences of elements of content, but since they also furnish the basis of that analysis and explanation, they seem to lead to a logical impasse. On analysis, the physical world is made up of mathematical and logical orders. But to argue, therefore, that the mind must be physical is to start a vicious circle which is completed by Bergson's (3) argument that the physical world has these characters only because of the structure of intelligence. They are in mind because they are in the physical world, because they are in mind, *ad infinitum*. In truth this order forms an argument for neither side. If the hypothesis of mathematical and logical organization of the physical world will account for the other attributes of consciousness, then it follows that mathematical and logical order must also rule consciousness, that logic is limited by the nature of material; as does the inverted argument of Bergson. The character of logical order therefore does not present evidence for the distinction of mind and body. If physical postulates fail to cover both logical and sensory sequences, they must fail for each as for the other.

Certain other relationships within the organization of consciousness seem to be less clearly implied in the postulates of the physical sciences. Elements known together may be compared, and yet retain their individual discreteness. This involves processes which are not obvious among physical events. But descriptive psychology finds in these processes only successions in content. Comparison, analysis, and the

like are but names for the fact that succeeding elements are determined by the sum of preceding elements. Introspection discovers unvarying sequence (determination) but the manner of this determination is undefined. Each of several elements may be followed by a specific sequence constituting introspection of, or thought about that element; or the elements in combination, under different conditions sometimes indefinable subjectively, may be followed by different sequences. This is all that is subjectively discoverable concerning the process of comparison.¹ A number of elements may be integrated in the final outcome, but the dynamics of integration is not open to introspective study. Explanation of the process demands postulation of mechanisms or processes underlying the successions of experience. The problem is as to whether physical mechanisms are adequate to account for all sequences which appear in consciousness.

Continuity of activity and sequence of events are included in the postulate of a physical world. To justify the setting apart of a psychic world it is necessary to show that the sequences of mental states differ either in the character of succession or in the results accomplished, from any sequences of the physical world.

The sequences and functions of thought are complex and difficult to state briefly, since they involve all the elaborations from day-dreaming to creative intelligence. For discussion we may divide them roughly into three overlapping classes.

1. The relatively unordered drift of reverie. Here elements follow each other by rather superficial associations (habitual connections which lack complexity of organization), or through common association with some emotional background, though the elements themselves may seem otherwise unrelated.

2. The reproduction of habitual sequences, as in the flow of memorized material or making of habitual judgments. Subsequent elements are rather simply conditioned by pre-

¹ Such introspective accounts of comparison and generalization as that of Fisher (9) which attempt to give an exact description of content without metaphysical interpretation clearly bear out this contention that sequences alone are discoverable by subjective methods.

ceding ones, the whole dominated by an as yet undefined close organization of the system, represented by the 'set' for reproduction.

3. Creative thinking involving a problem set and a solution reached.

This is essentially the classification given by Watson (32), except that habitual sequences seem to me to involve a closer and more complex organization than do the sequences of reverie. The first two classes present no new problems beyond those discussed under the attributes of content, save the determination of sequences. Subjective evidence gives no explanation of this determinism, but is forced to fall back upon the hypothesis of physical continuity. 'Aufgabe' and the like describe no causes whose mode of action can be understood, and in many cases the introspectionists confess that the determining tendency is wholly unconscious. There is determination, but no particular kind of determination and there is not subjective evidence to show that the determining tendencies may not be wholly physical.

The third class presents the supposedly creative work of consciousness. Subjectively, the problem seems to present three phases; determination of sequences, conflict of elements of content, and resolution of the conflict. I can make these points clearer by a concrete example.

I am confronted by a mass of stimuli—notes of experiments, histological specimens, charts, etc. My scientific training results in the habitual reaction to such masses in the setting of my laboratory by the question, 'What is it all about?' and by a feeling of dissatisfaction until an answer is given. The data are neurological.

Destruction of the frontal lobes—loss of habit.

Habit relearned after destruction.

Incomplete destruction of frontal lobes—habit retained.

Destruction may involve any half—habit retained.

These data are given, partly in verbal terms, but largely in kinæsthesia. There is, in addition, a feeling of tension, of movements, which, if completed, lead to gestural description of the data, but which for the most part seem in conflict with

other gestures. This is all that is subjectively present of a 'purpose' to solve the 'problem.'

Associations come: frontal lobes—attention—Pillsbury—attention necessary for learning—learning impossible in absence of attention center—possible in absence of frontal lobes, increased feeling of dissatisfaction and dropping of this line of association.

This presents the problem of logical conflict, a mutual incompatibility of ideas.

I start again, parts capable of doing what the whole does. (This appears as a somesthesia of wabbling in three dimensions and during the problem solving it has no other meaning. When I return to it and introspect, it is followed by memories of my solution of Driesch's inconceivable machine, as a lazy-tongs reduplicated in three dimensions.) Driesch—violent emotional reaction with vague memories of discussions of vitalism—sensations of shrugging and raising upper-lip, abandonment of this line of association.

This presents failure of solution through emotional conflict.

I start again, lazy-tongs—multiplication of identical parts—identical parts in central nervous system—feeling of hands raised with spreading fingers—fibers to cortex—one hand down—part destroyed, remainder functioning. Here follows a relief from the initial dissatisfaction which constituted the problem.

The problems presented here are those of tension or conflict and relief from the tension. Subjectively the tension is nothing more than feeling of muscular tension and emotion. I tend to interpret it as an interference of two incipient acts which are incompatible (*i.e.*, 'up' is incompatible with 'down' because the feeling of raising of the head which is 'up' is interfered with by the feeling of bending the head which is 'down'.) But the incipiency is an interpretation from the fact that in subsequent introspection I find either or both of the two acts carried out independently. It is the old fallacy of inferred meanings.

Subjectively, the problem of creative reasoning reduces

to feelings of tension, determined sequences broken off after more acute tensions, and final subsidence of the initial tension. This may not be recognizable as a description of the solution of a problem, but the further characteristics usually demanded of such a description are teleological interpretations and not elements of the experience of problem solving.¹ The first tension we call 'set' because of its consequences. The sequences we call successful or unsuccessful trials, in view of the outcome. The correct solution differs from the incorrect only in its further consequences in behavior or mental content. In the process there are no attributes, save those of static content, other than the attributes of the physical world. The description of a rat opening a problem box is as complete an account of the *process* of thinking as can be given from introspective data.

In this analysis of the attributes of consciousness, I have attempted not to overstep the point of view of the subjectivists and to adhere to their terminology as far as possible. I have sought to discover, further, just what the unique features of consciousness are thought to be, to strip them of their mystical obscurity and put them in definitive form. On subjective evidence, nothing can be said as to how one idea leads to another, nothing as to why assent or dissent is given. The dynamics of thought is not an object of awareness. The goal in problem solving is no more evident in preceding contents than in the goal of evolution in the existing species of animals. Both can be known only when reached. If behaviorism can formulate any mechanistic account of accomplishment in problem solving, it will have fulfilled the subjectively definable requirements for conscious purpose and for the creative action of consciousness.

This brief analysis of the attributes of consciousness necessarily omits many considerations of importance for the complete development of the behavioristic argument, but I believe that it will indicate the direction which that argument

¹ In his recent criticism of behaviorism Pratt (24) has overlooked the fact that the introspective account of a purpose (for example) must be just as unintelligible to the philosopher as the behaviorist account unless it also is tagged with the name current in philosophy.

may safely take. The physical sciences deal with postulated entities having certain attributes and relations. Granting the validity of their system, we seek to extend it, without fundamentally modifying its postulates, to include the phenomena upon which the concept of mind, as distinct from the physical universe, is based. Analysis of these phenomena shows that in so far as they are definable on introspective evidence they consist of varying, complicated organizations of elements within a limited system; the elements themselves being definable only in terms of their relationships within the system. The behaviorist's problem is to describe this system in terms of the conceptions of the physical sciences; to show that relationships such as are ascribed to consciousness exist also among physical entities.

III. VITALISTIC ARGUMENTS

I have thus far dealt with the view which maintains that there is evidence of a direct experience of a universe of psychic things which is fundamentally different from the universe of physical things. There remains another type of argument against behaviorism which holds that certain events in the physical world are inexplicable in terms of mechanism. This is the argument of vitalism, as distinct from the first or animistic argument. The vitalist cites particular phenomena—morphogenesis, regeneration, habit-formation, complexities of speech, and the like—and denies the possibility of a mechanistic account of them (6, 20). But he thereby commits what we might term the egotistic fallacy. On analysis his argument reduces every time to the form, "*I am not able to devise a machine which will do these things; therefore no one will ever conceive of such a machine.*" This is the argument from inconceivability of Driesch and McDougall, put baldly. To it we may answer, "You overvalue your own ingenuity." But the real answer is the constant restriction of field which science is imposing upon vitalism. A few years ago the impossibility of a physicochemical explanation of secretion against an osmotic gradient was a favorite vitalistic argument.

Recent work in physical chemistry has given an adequate

explanation of the phenomenon in terms of electrical energy produced by adsorption in membranes and has led to the construction of a machine which actually secretes against an osmotic gradient. Such is the answer of physical science to vitalism. Science has not yet explained the physical world, but the vitalist cannot, by taking thought, set limits to what it may explain.

A second anti-mechanistic argument is typified by Haldane's discussions (11). It is apparent also in certain attempts of some behaviorists to distinguish between their science and physiology (36). Haldane's argument is essentially the following. Physiological investigation reveals more problems than it solves. We can never hope to give a complete account of the organism in physical terms. All investigation, however, must be directed toward this end, and attempts at other explanation, as by introducing the concept of vital force, are futile. But since we can not hope for a full explanation of the behavior of the organism we must add to the mechanistic account the conception of the *organism* in physiology, and of the *personality* in psychology: wholes which are more than the sum of their parts.

I can not see in such discussions anything more than a warning against too great simplification of our explanations. Obviously the various physiological processes influence each other throughout the organism. But astronomy equally recognizes the influence of the farthest star upon the smallest atom in the earth and consequently admits the incompleteness of its account of the universe. Organization, in this sense, is no more a property of living things than of the non-living.

Of such objections to the formulations of behaviorism there can therefore be no criticism, so long as they remain simple warnings, but they seem inevitably to lead to an abandonment of the search for physiological explanation and to the substitution of empty names (the organism as a whole, regression of the stimulus, personality, and the like) for explanation. They seem to lead also to such statements as, "We must consider the social value of the stimulus in relation to the organism," as though social value had other existence

than in the reactions of the organism. And because of this tendency to replace explanation by name and to read into the names mystical potentialities, I must object to any definition of behaviorism which would make it more than the science of the physiology of reaction to stimulation.

The discussion of a third anti-behavioristic doctrine, which emphasizes the humanistic values of subjective psychology, I shall leave to a later section of this paper.

Thus far in the discussion I have sought to state the distinguishing attributes of 'mind' as the subjectivist must define them on the basis of the empirical evidence of introspective analysis. Too often in discussions of the behavioristic doctrines the impossibility of an account of consciousness in physical terms is asserted with no adequate analysis of the supposedly distinguishing features of 'mental' phenomena. To consider a specific instance: Lovejoy (18) says that the error of the behaviorist is easily demonstrated on his own premises, "For a behavioristic psychologist (a) is a human organism, (b) whose perceiving and thinking, if his own theory is correct, should be exhaustively describable in terms of movements of his laryngeal and related muscles, but who (c) in fact thinks, or professes to think, of external objects and stimuli, that is, of entities outside of his body, (d) which thinking is obviously neither describable as, nor 'accounted for' by, movements of his laryngeal or other muscles inside his body." Now to the behaviorist his thinking is just as *obviously* so describable as it is indescribable to the subjectivist. The obviousness in either case arises from a background of metaphysical prepossessions, in this case the belief in transcendence of space. Such conclusions are not self-evident; the premises demand further analysis and citation of evidence.¹ If we accept the subjectivist's postulate that mind presents things-in-themselves which are, by definition, not describable in physical terms, or relations which are not of the physical world, then *obviously* they are not describable in physical terms.

But examination of the empirical evidence shows that

¹ Cf. Warren's criticism (29) of Lovejoy's discussion.

many of the attributes ascribed to consciousness are not discoverable by introspection and that others, when cleared of the mysticism that has surrounded them and stated in terms of descriptive psychology instead of metaphysical interpretation, are not different from characteristics resulting from physical relationships. Our analysis has shown that 'mind' is definable in terms of certain kinds of relationships among elements which are not analyzable by introspection. In the following sections of this paper I shall try to show that these relationships are fully describable in terms of the attributes which the physicist and biologist ascribe to the physical world with which they deal. My thesis will be, primarily, that as complete an account of the attributes of consciousness can be given in behavioristic terms as can be given in subjective terms as a result of introspective study; that a description of behavior of the physiological organism shows just those relations and elements which are held to characterize consciousness. In other words, I shall try to show that the statement, 'I am conscious' does not mean anything more than the statement that 'such and such physiological processes are going on within me.'

(To be concluded)

PURPOSIVE OR MECHANICAL PSYCHOLOGY?¹

BY WILLIAM McDougall

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It is my privilege to deliver before you a single lecture only, in the midst of a systematic course of lectures on Psychology. It would hardly be possible for me to fit this lecture into the scheme of this course, to make it an harmonious part of the whole, filling what would in its absence be a gap in that course. Nevertheless, I hope that I may render you a service, may deliver a lecture which may not be wholly unprofitable. For Dr. Watson, who presides over and has planned your course of instruction, has wisely judged that you, being men and women of mature minds, may benefit not only from having presented to you in detail his own vigorous Psychology, but also from being made acquainted with other and rival views, other views of the most profitable way to describe human nature and explain human conduct.

For, unfortunately, psychologists are still very widely divided as to the proper methods and aims of their science. And it is only right and fair to you that you should be made aware of these deep-lying differences, and should have the issues presented from the different points of view of those who take different sides in regard to them.

You can hardly fail to know that Dr. Watson is the leader of a school of thought in psychology which at the present time flourishes like a green bay tree, I mean, of course, the school of 'behaviorism.' I may assume that you understand what this word 'behaviorism' implies, what are the essential peculiarities of the academic species, behaviorist. You know also that Behaviorism is acutely opposed to Introspectionism, the view that introspection is for psychology an important and indispensable procedure. This rivalry

¹An address to students of the psychology class of the New School of Social Research.

between Behaviorism and Introspectionism is one of which we hear much at the present time; for it is the topic of a very lively controversy. And the issue is very important; but it is not the most important and deep-lying of the questions on which psychologists are divided. A deeper lying issue, one of more fundamental and more permanent importance, is the one denoted by the title of my lecture, namely,—"Purposive or Mechanical Psychology?"—the one on which I propose to take a side very definitely this evening, as an advocate of Purposive Psychology.

This more fundamental question is entangled with that between Behaviorists and Introspectionists; for the Behaviorists are, with few if any exceptions, Mechanists or advocates of the Mechanical Psychology. And it may, I think, be said without serious error that most of the Behaviorists have been led to their peculiar view by way of the mechanistic position, that they have been Mechanists first and Behaviorists secondarily only. It is theoretically possible for a Behaviorist to be an advocate of the Purposive Psychology; but I am not able to name any prominent psychologist who actually and unambiguously represents the combination of these standpoints.

The relations of Introspectionism to the more fundamental issue are logically similar; that is to say the Introspectionist may stand for either the Purposive or the Mechanical Psychology. But in practice the relations are more complex; for the Introspectionists, far from being wholly or predominantly either Mechanists or Purposivists, are about equally divided upon this issue. Actually, then, we have psychologists falling into three main groups; first, they fall into the Mechanical and the Purposive groups; and the Mechanical group in turn consists of two divisions: the Behaviorist Mechanists and the Introspective Mechanists.

It will aid you to envisage this divided state of opinion, if I sketch very briefly the way in which it seems to have arisen. The history of the division goes back at least as far as Descartes, the seventeenth century father of modern philosophy. In his time and largely by his efforts, the con-

ception of mechanical process as a sequence of strictly mechanical causes and effects first became clearly differentiated from the conception of mental or purposive activity.

It had always been recognized that men are thinking beings, who act purposively, who become aware of things and of one another, who feel and desire and strive to achieve the goals that they imagine and desire. And the animals also had commonly been regarded as similar beings of simpler constitution; though as to this there were wide differences of opinion. Descartes suggested the view that all the animals are purely machines; that all their actions are the outcome of purely mechanical processes going on in their bodies; that, therefore, there is no need and no justification for supposing that they in any sense could think, could be conscious, could feel or desire or strive. And he suggested the principle of reflex action as one by aid of which all their bodily movements, all their responses to stimuli, could be adequately explained. Men, on the other hand, he regarded as similar machines, but machines endowed each with a soul or mind or thinking principle, which exerts an intelligent, conscious, purposive guidance upon the mechanical reflex processes of their nervous systems. Descartes' conception of mechanical reflex action, as the explanation of all animal behavior, was not only extremely ingenious and plausible; it was also very acceptable to the Church, which had felt some difficulty about the attribution of souls to animals, and which accordingly welcomed a scheme that abolished all reasons for such attribution. Hence this mechanical theory of animal behavior was widely accepted; for the Church did not foresee that its acceptance would almost inevitably lead to the extension of the mechanical theory to human behavior also, to the denial of souls or minds to men. But so it was. The studies of the physiologists have confirmed and refined Descartes' conception of reflex action; until in our own day the brain has been made to appear as nothing but a vast automatic telephone exchange, in which the messages from the sense-organs are mechanically switched into appropriate motor nerves, thus determining appropriate movements in

response to each of a multitude of sense-impressions or sense-stimuli. Hence the intelligent purposive operator, whom Descartes imagined as presiding over the switch board, has been pronounced an unnecessary hypothesis. This conception has now been formulated as the foundation stone of the creed of the Mechanical Psychologist. The formula runs as follows: Every human action is a mechanical reflex response to a stimulus.

This formula would hardly have gained wide acceptance, but for the fact that two developments of modern science seemed to harmonize with it and to confirm and demand it. One of these developments was the increasing definiteness and confidence with which the mechanical sequence of cause and effect was conceived and the conception generalized and applied to every type of process studied by the physical sciences. This development culminated, about the middle of the nineteenth century, in the formulation of the law of conservation of physical energies. This law seemed to render possible a deductive justification of the Mechanical Psychology. For, it was argued, the human body is a material system subject to all the laws of physical science. The processes of the body that issue in bodily movements are no exception; they are the play of physical energies subject to the law of conservation of energy; therefore it is impossible that mind or intelligence or consciousness, which are not physical energies, should in any way modify or guide or interfere with the play of these physical energies proceeding according to purely mechanical laws. In this way there grew up in the minds of many scientists a belief or prejudice which may conveniently be called 'the mechanistic dogma,' the belief that every process in the world is of the mechanical type, in principle explicable by mechanical laws.

The other development, making in the same direction, took place within psychology itself. Locke and Hume introduced the fashion of describing all thinking, all consciousness, all experience, as a stream of 'ideas,' which ideas are strung together according to a single principle, the principle of association; and David Hartley, who was their immediate

successor, showed how the brain-processes might be regarded as the scene of operation of this principle of association. Then other psychologists undertook to describe more exactly what these 'ideas' are made of; and fifty years ago they greatly refined their procedure for the analysis of 'ideas,' by carrying on the process of introspective analysis under experimental conditions in laboratories furnished with all kinds of ingeniously designed instruments. This minute analysis has led many of them to the conclusion that each 'idea' is a group or cluster of elements of consciousness which they call 'sensations.' The study of these 'sensations' was then combined with the study of the reflex processes of the brain; and it was made to appear that each sensation is the effect, the immediate consequence or accompaniment of, one elementary brain-process or reflex; and that a sensation-cluster or 'idea' is the accompaniment of a more or less complex group of such brain-processes. How the mechanical reflex process of the brain calls into being its appropriate sensation remained a mystery, a problem with which science as such was not called upon to deal.

Thus, by the convergence of these three lines of modern science, the development of the mechanical conception of all natural processes, the study of reflexes, and the study of 'ideas' and 'sensations,' the foundation was laid for the contemporary Mechanical Psychology. T. H. Huxley first definitely brought the converging threads together in his doctrine of 'Epiphenomenalism,' the doctrine that our thinking or 'consciousness' is the epiphenomenon of the mechanical processes of the brain. This formulation has been widely accepted by psychologists; and though some of them have varied the formula of the connection of 'sensations' with the brain-processes, it is roughly true to say that it has been accepted by all the psychologists of one of our three groups, namely, all those who continue to make use of Introspection, while accepting the mechanistic dogma and the Mechanical Psychology.

It remained only for your guide, philosopher, and friend, Dr. J. B. Watson, to take the ultimate step in this direction. Dr. Watson, contemplating the differences of opinion and the

interminable controversies among psychologists about the 'stream of consciousness,' and the many different ways of analyzing it into 'ideas' and 'sensations,' boldly proposed to cut the Gordian knot, to banish all these perplexing apples of discord into the limbo of useless questions which need never have been raised. Why trouble ourselves, he said, over these insoluble questions about 'sensations' and 'ideas?' We shall never solve them; and, if we did, we should be no better off. What we, as psychologists, want to understand and to control is human behavior; and since all human behavior is but the mechanical reflex response to sense-stimuli, let us confine ourselves to studying these stimuli and these responses, together with the structure and functions of the nervous system. Let us leave all that has been called 'consciousness' with its 'sensations' and 'ideas,' if indeed they are not purely mythical entities, to the metaphysicians. Dr. Watson has been followed by a large number of the younger psychologists; and so he has split the party of the Mechanical Psychologists into two groups, those who continue to use introspection, holding that by so doing they may throw further light on the all-important mechanics of the brain, and those who hold with him that nothing further is to be gained by such introspective study.

Now let us go back a short step in this history, and see how the culmination of the tendencies we have noticed in a definitely and explicitly Mechanical Psychology may give rise to a different consequence. Behaviorism is the consequence of carrying the mechanizing tendency in psychology to its logical conclusion, and indeed to a point some distance beyond its logical conclusion. The other consequence was of a very different nature. It results from stopping to think, instead of pursuing the headlong course of deduction from the mechanistic dogma. Confronted with the conclusion that Man is but a penny-in-the-slot machine, differing from other familiar examples of such machines only in that he has many slots adapted for the reception of pennies of as many different sizes and shapes, each releasing a response peculiar to itself—when confronted with this conclusion, it is surely high time

to stop and think a little, and to examine our reasoning and our premises. And especially does it appear necessary to re-examine our reasoning, when we find that it has led us to a kind of psychology which, strictly speaking, is quite useless.

I can best illustrate this alternative consequence of the culmination of the mechanical tendency in psychology by citing the case of a very eminent and influential psychologist who has recently passed away. I mean the late Professor Münsterberg. Münsterberg was a vigorous and logical thinker, who, having been carried along with the predominant tendencies of his time, found himself teaching and defending vigorously and even somewhat dogmatically the Mechanical Psychology of reflexes and attached sensations. But he differed from many of his colleagues in that he clearly saw that this psychology was an artificial construction, reached by processes of abstraction from the concrete reality of our mental life and conduct, and that it gave neither a description of human nature nor an explanation of human conduct. For Man is a volitional purposive being, and his conduct is the expression of his desires and of his will; and, as Münsterberg clearly saw and repeatedly asserted, his desires and his will escape entirely the purview of the Mechanical Psychology. And Münsterberg, with unflinching logic, wrote a series of trenchant essays showing irrefutably that the Mechanical Psychology can have no bearing upon, and no application to, the problems of human life, the problems of voluntary conduct, the acts of men proceeding from desire and will. These problems, he declared, belong, not to psychology, but to philosophy; he even denounced, as anti-psychological and as pseudo-psychology, every attempt to treat of such problems scientifically.¹ And he issued grave warnings against the folly of attempting to apply the principles of Mechanical Psychology to the solution of the problems of real life, the problems of education, of politics, of history, of ethics.²

¹ In his 'Grundzüge der Psychologie' (1900) and in the volume of essays 'Psychology and Life' (1899).

² I have not been able to find any one passage which frankly and roundly asserts that mechanical psychology is completely useless, in the sense that it has no bearing upon the problems of human life; but that is the tenor and substance of the two

Nevertheless, Münsterberg continued to pursue and to defend the Mechanical, or, as he preferred to call it, the Causal Psychology. For, he said, this is the only kind of psychology that is scientific; it is an artificial transformation or translation of the facts of our mental life into the terms of physical science, made by the psychologist with the deliberate purpose of rendering an account of our mental life in terms of mechanistic science, which, he declared, is the only kind of science that is possible. But at the same time that, as a scientific psychologist, he described our mental life as a stream of sensations mechanically linked by the reflex processes of the brain, he declared that, as a philosopher, he believed in the reality of purposive effort, in the freedom of the will, the creative activity of the mind and the immortality of the soul.

Thus two voices dwelt in the breast of Münsterberg, that of the philosopher and that of the scientific psychologist, issuing in clear and confident tones two different and utterly incompatible accounts of human nature. In all his voluminous writings of this period, Münsterberg failed to provide any satisfying answer to two questions which his many disciples and admirers might well have put to him. First, they might well have shown some curiosity as to the relations

books mentioned in the foregoing footnote. I cite from 'Psychology and Life' a few passages which assert this doctrine most explicitly: "Psychology thus presupposes for its purposes a most complicated transformation of the reality [of our mental life], and any attitude toward the mental life which does not need or choose this special transformation may be something else, but it is not psychology. Practical life and history, mental science and poetry, logic and ethics, religion and philosophy, all deal with mental life, but never with psychology as such" (p. 112). Referring to child psychology, experimental psychology, and physiological psychology, he wrote: "We must now ask of what use they are for the individual teacher. My answer is simple and is the same for all the three branches: I maintain that they are not of the slightest use" (p. 128). "Certainly the teacher ought to study children and men in general, but with the strictly anti-psychological view" (p. 132). "Psychology and history must never come together again" (p. 186). "Psychology and history have thus absolutely different material . . . and thus they are separated by a chasm" (p. 205). "Psychology and history cannot help each other and cannot interfere with each other so long as they consistently stick to their own aims." "I have sought again and again to point out this unfortunate situation, and to show that history and practical life, education and art, morality and religion, have nothing to do with these psychological constructions, and that the categories of psychology must not intrude into their teleological realms" (p. 264).

between these two voices, these two men within him, the mechanical psychologist and the free philosopher. Did they never commune together? Did they never conflict? Did neither one ever question the validity of the other's utterances? Secondly, Münsterberg's disciples might fairly have asked of him—Why, if your scientific Mechanical Psychology is so perfectly useless, having no bearing on problems of human nature and conduct, why do you practice and defend and promote it? But these questions were never asked. Or, if they were asked, they evoked no satisfactory reply.

It is true that Münsterberg repeatedly affirmed that the transformation of our mental life effected by mechanical psychology is "logically valuable" and that he, as a philosophical psychologist, made this transformation with deliberate purpose. "Psychology," he wrote, "may dissolve our will and our personality and our freedom, and it is constrained by duty to do so, but it must not forget that it speaks only of that will and that personality which are by metamorphosis substituted for the personality and the will of real life, and that it is this real personality and its free will which create psychology in the service of its ends and aims and ideals" (*op cit.*, p. 23). And in a later work ('Psychology, General and Applied') he wrote: "Our mental life is free, and through an act of freedom we decide to consider it as a mental mechanism in which nothing is free" (p. 296). And he insisted again and again that the psychologist has a right to do this, to elaborate, that is to say, a distorted and utterly false and misleading picture of man as a machine. Well! This is a free country, and academic freedom is the accepted rule; and so the psychologist has the right to use his freedom in order to pretend that he is a machine working strictly according to the laws of mechanism, if he perversely finds amusement in doing this. But our question is—Why should he want to play this strange game? What will it profit him? And above all, why should he claim such perverted activity as not only the right but also the duty of the psychologist? In the 'Psychology, General and Applied,' which belongs to Münsterberg's third phase, he offered a more definite answer to such questions than any I can find in the writings of his first or exclusively mechanical phase. "He [the causal psychologist] wants to understand the inner life too as a system of causes and effects and to recognize every experience as the necessary result of foregoing conditions, in order to foresee what will happen in the mind and to influence it. If this is the purpose, any reconstruction of the inner life which helps toward this goal must be welcomed as psychological truth; but it must not be forgotten that it is indeed a reconstruction and not original life reality" (p. 291). But if the mechanical description of human nature and action involves, as Münsterberg correctly asserts, a tremendous transformation of the reality, what right have we to expect to be able to 'foresee what will happen in the mind and to influence it' on the basis of this deliberately falsified description? And that the transformation and falsification are very serious Münsterberg leaves us in no doubt. Referring to this transformation by the causal psychology in describing emotion and volition, he wrote: "We did it there as if we found in those combinations of bodily sensations the real emotions and feelings themselves. Looking backward from a higher point of view we must recognize that all those introspective observations were ultimately remolded constructions. They were needed, because they alone allowed us to treat the functions of the self as describable

objects and to link them in the chain of causal events. The results, accordingly, were psychological truths, but they certainly led us far away from the immediate reality of inner life. It is this reality which must be analyzed and systematized by the purposive psychologist. . . . The contrast between the purposive interpretation and the causal description of the personal and interpersonal life is complete at every point" (p. 292). And again—"it is our purpose which transforms our purposive life into a causal structure" (p. 295). Yet again, referring to the description of mental life given by the mechanical psychology, he writes: "This is a tremendous transformation of reality and nothing is more surprising than that it no longer surprises us" (p. 289). On which one is tempted to comment that as psychologists we know that we can soon accustom ourselves to almost anything, and that the persistence of so many psychologists in making this tremendous transformation is a matter not for surprise but rather for depreciation and protest. As to Münsterberg's claim that the descriptions and explanations offered by mechanical psychology are "psychological truths," the claim may be allowed, but only in the sense in which the claim is made, namely, that the statements offered are consistent with one another and with the rules of the game deliberately adopted in the service of this purpose; that is to say, in the same sense in which the statements of any well told fairy story are true, namely, in the sense that they conform to the rules of fairy-story-telling and hang consistently together to make an amusing story.

This was Münsterberg's first phase, the first of three. Soon after he had made these pronouncements and had issued his warnings against all misguided attempts to apply psychology to human problems, he became involved in a series of brilliant researches which attracted widespread public interest, researches directed to throw light upon certain urgent problems of industry and of law. These researches did much to hasten the application of methods of the psychological laboratory to practical problems of education, of industry and of law, methods which at the present day are so widely practiced in those fields; and they justly added much to the fame of Münsterberg. This was his second phase, a period of some years, during which with marvelous energy he poured out a stream of books on the applications of psychology to almost every branch of practice concerned with human nature.

Then he turned again to make a systematic presentation of his views and wrote shortly before his death, as the first and sole product of his third phase, his 'Psychology, General and Applied.' It was obvious that some reconciliation, or at least some *modus vivendi*, was required between the teachings of his first phase and those of his second. How did he effect

this? It might have been supposed that, in the light of the experiences and results of his second phase, he would have recanted all that he had said on behalf of the Mechanical Psychology. But no, he was not ready for that—that might have come as the teaching of a fourth phase, had he lived ten or twenty years longer. In his third phase he contented himself with a compromise which was obviously unstable and unsatisfactory. It was of this nature. He divided his treatise into three sections or books. The first presented the Causal, the Scientific, the Mechanical Psychology of his first phase. The third presented the Applied Psychology of his second phase. But since, according to his own oft-repeated declarations, the account of human nature rendered by the Causal Mechanical Psychology was so remote from reality, so transmogrified, that it could not be used as a basis for Applied Psychology, he provided that basis by writing on fifty-three pages the 'Principles of Purposive Psychology' constituting Book II of the treatise. And now, in place of describing such treatment of human nature as anti-psychological, he wrote: "The first step in purposive psychology is to analyze the tissue of purposive life into its elementary acts. . . . These purposive acts are the most familiar experiences which we know. We must always begin with such an analysis where we want to understand an individual."¹ And he said: "The careful research with all the aid of experimental and comparative methods may just as well be devoted to the purposive aspect of mental life."² That is to say, in this third phase he no longer described every attempt to take account of the purposive nature of Man as unscientific and anti-psychological; rather, he admitted not only that such treatment of human nature might be scientific, but also that it might be furthered by experimental methods of research.

In this third phase, then, Münsterberg wrote and taught two distinct and very different psychologies, accepting both of them as legitimate and as scientific. He continued to approve and to teach the Mechanical Psychology, out of deference, I suppose, to the customs established by the sug-

¹P. 321.

²P. 313.

gestive power of the mechanistic dogma; in spite of the fact that he recognized it to be purely an academic game, played according to artificially prescribed rules and merely for the purpose of conforming to the prevalent prejudices in favor of purely mechanistic science.

In reference to the rise of the Mechanical Psychology Münsterberg wrote: "When this new endeavour to study the special facts of the mind awoke, the natural sciences were at the height of their success, and their suggestive power forced the naturalistic method on the empirical psychologist." *Op. cit.*, p. 312. Here we seem to have the true answer to the question we asked just now—Why does the causal psychologist want to represent man as a machine? And the answer is—By reason of the suggestive power of the natural sciences and of their mechanistic dogma. Thus in his third phase Münsterberg supplied the true and simple answer to the question raised on a preceding page, namely—How are we to explain the fact that the mechanical psychology has had so many adherents? In his first phase Münsterberg elaborated at great length a philosophical explanation and justification of the fact. In his third phase he gives the answer in one sentence in terms of the purposive psychology—"the suggestive power of the natural sciences forced the naturalistic [*i.e.* the mechanistic] method on the empirical psychologists," or on many of them, of whom Münsterberg was one. I cannot find any passage in which he applies the adjective 'scientific' to the purposive psychology. Nevertheless it seems clear that in this third phase he regarded it as equally scientific with his causal psychology. For, in place of denouncing the purposive account of human nature as anti-psychological and pseudo-psychological, he gave it a place in his volume as a necessary buffer between his useless or inapplicable causal psychology and his applied psychology. And he wrote—"This science of the purposive behavior of the soul may bring together in future just as many special facts as our handbooks of causal psychology can marshal to-day." (p. 313). And in this third phase we find, in place of the assertion that history and psychology have nothing in common and must never come together, such statements as—"the historian must speak the language of purposive psychology" (p. 333)—and, "It is no less fitting and natural that the progress of psychology, too, should become helpful to the community wherever mental life is involved in its affairs, and it is evident that the mind takes a characteristic part in every domain of social interest, of education and of religion, of politics and of law, of commerce and of industry, of art and of scholarship, of family life and of practical intercourse, of public movements and of social reform" (p. 342). This implies a 'tremendous transformation' of the views expressed so elaborately fifteen years earlier. Yet curiously enough Münsterberg did not in his third phase explicitly recant any of the teachings of the first phase. Rather he still sought to keep up the appearance of consistency between the teachings of his three phases, and hankered after some justification, some *raison d'être* for the causal psychology; and so we read: "We must be able to foresee what will happen and to determine how we can mold the mind. In short, if we want to treat man as a means for the realization of our practical ends, we must have causal psychology. Through the practical application above, the unnatural problems of causal psychology become justified. The technical use of psychology gives real meaning to the whole great movement devoted to the causal study of the mind. If this end did not exist, causal psychology would be a scholastic attempt to solve an unnatural problem" (p. 346). This failure frankly to admit a change of view and the persistent hankering after some justification of

the causal psychology involved Münsterberg in a sea of inconsistencies and contradictions. The 'causal psychology,' which in his first phase he had shown to be incapable of rendering service to the sciences concerned with human nature, he claims in his third phase to be the scientific basis, not of all applied psychology, but of what he calls 'psychotechnics,' the "practical application which aims toward the realization of certain concrete ends as against that other applied psychology which simply explains the given historical facts. Then the psychotechnical sciences stand in contrast to the psychohistorical sciences. Psychotechnics is really a technical science related to causal psychology as engineering is related to physics. Psychotechnics necessarily refers to the future, while the psychohistorical sciences refer to the past." We are left to infer by a process of exclusion that purposive psychology is the basis of the psychohistorical sciences. For in this third phase history and psychology are no longer separated by an impassable chasm; rather, we are told, "the historian needs his psychology as the physicist needs his mathematics" (p. 356). But he has asserted again and again that causal psychology explains human thought and action, and explanation is necessarily and always causal in the sense of mechanistic or mechanical, and necessarily ignores purpose and will. Yet now he tells us "the psychotechnical endeavors may be turned in any direction in which important purposes of man are to be fulfilled. If we classify psychotechnics, we ought to divide it according to the groups of human purposes" (p. 354) and "psychotechnics is confined to the region of actual human purposes" (p. 355). On the other hand he has asserted again and again that history and the other studies concerned with human thought and action in its purposive reality must never seek to explain such actions but only to interpret and understand them; yet now he asserts that the psychohistorical sciences must "explain the concrete happenings in mankind" (p. 355). In short, Münsterberg insisted that causal explanation and purposive understanding are two radically different ways of treating of human events. In his first phase, the way of 'causal explanation' was alone scientific and psychological, the other was anti-psychological and pseudo-psychological. In his third phase, both ways are asserted to be scientific and psychological; and he might have achieved consistency and clearness in his new position by frankly standing by his former declarations that the causal psychology is incapable of application to any real problems of human life, and by frankly admitting that he had now changed his view and that the purposive psychology is alone capable of such application. Instead of doing so, he sought to preserve an appearance of consistency with the teachings of his first phase, at the cost of a hopeless confusion and inconsistency within his new presentation of the principles of psychology.

But though he accepted both these psychologies as scientific, Münsterberg recognized that of the two, the purposive is vastly the more important, the more worth while. He said of the Purposive Psychology that it is "not only the most immediate and most natural experience, but also the most significant way of looking on man. To treat man as an object, as causal psychology must always do, is much less important than to acknowledge him as a subject and to understand his meaning."¹

¹ *Op. cit.*, p. 312.

I have no time to put before you the argument which shows the truth of Münsterberg's declaration that the Mechanical Psychology is of no practical value, has no bearing on real life. It should be obvious at first sight; but for those who cannot see this truth on mere inspection, a long array of argument and evidence is necessary. I have set this forth elsewhere.¹ I will only point out in passing that the mechanistic dogma, on which the Mechanical Psychology is founded, is a faith and a hope rather than a truth; a misguided faith and an ill-grounded hope. In no single branch or corner of biology has it justified itself hitherto; and the more we know of organisms, the more improbable does it appear that the dogma will ever find justification. The behavior of even quite lowly animals defies mechanistic explanation and shows unmistakably the objective marks of purpose. I venture to predict that, a few generations hence, the wide prevalence of that dogma at the present time will be recorded by the historians of science as a strange aberration of the human mind, to be classed with sympathetic magic and the belief in witchcraft.

My purpose in this lecture is, then, to warn you against an exclusive devotion to the Mechanical Psychology. For certain very limited purposes, such as the minute introspective analysis of the processes of sense-perception, it may have a certain value, as providing a convenient, though misleading, terminology; and you may practice certain applications of psychology, such as intelligence-testing, or the measuring of skill, or the retentivences of memory, without troubling yourselves about the principles of psychology. But, if you turn from mere intelligence-testing to the study of personalities, even from the most strictly practical point of view, such study as is now recognized as of practical importance in the industrial field, then you must at once be deeply concerned with such qualities as energy, endurance, initiative, perseverance, foresight, ambition, prudence, and idealism. And all these are qualities which the Mechanical Psychology cannot take into the narrow framework of its "tremendous transformation of reality."²

¹ 'Body and Mind' and 'Outline of Psychology.'

² Münsterberg, *op. cit.*, p. 289.

If you want a psychology which will help you to understand the life of men in society, a psychology which will enable you to give aid to those in mental distress or disorder, to direct wisely the development of your children, to discuss profitably the problems of ethics, of politics, of economics, or of history, then you must acquire a Purposive Psychology. For in all these great fields of human interest you have to do chiefly and primarily with human volition, human desires, human ideals, in a word, with human motives; and the Mechanical Psychology knows nothing and can teach nothing about human motives. Let me give a single illustration of this truth from a field of very practical everyday psychology. Any day, I suppose, any one of you may be called to serve on a jury to try a criminal charge. Suppose it to be a charge of murder. One man has shot another and killed him. The testimony of witnesses establishes beyond doubt that the prisoner held the pistol and fired the fatal shot. The witnesses give you a full behavioristic account of the incident. From this you may be able to conclude with confidence that the shooting was not an accident; that the prisoner *intended* to shoot his victim. There you have reached from the description of behavior a conclusion which goes beyond the purview both of Behaviorism and of the Mechanical Psychology; for neither of these can take account of intentions. But the court will be interested in a still more difficult psychological problem, namely—Granted that the prisoner intended to shoot his victim, did he intend to kill him? The fullest possible behavioristic account would fail to throw light on this question.¹ But there is a still deeper psychological problem which the court must solve, before it can do justice to the prisoner. Granted that his intention was to kill his victim, what was the motive of that intention? There you have the most essential problem of the case, and one before which the Mechanical Psychology, whether of the behaviorist or the introspectionist variety, is perfectly helpless. For to

¹ It seems clear that for behaviorism the distinction between 'intending to maim' and 'intending to kill' can have no existence. Mechanical introspectionism might, I suppose, admit the distinction and describe the difference in terms of kinæsthetic sensations.

both of them the word 'motive' is meaningless. They can take no account and give no account of motives. Motives and purposes hang together; and only the Purposive Psychology can attempt to understand, to describe, or in any sense to explain them.

The evolution of Hugo Münsterberg (and my own evolution has been similar to his) should warn you against spending the best years of your lives in acquiring a kind of psychology that will be of no use to you, beyond perhaps enabling you to pass examinations. Life is short, and science is long. Be warned by our fate; be converted early to the true faith; turn now to the study of Purposive Psychology.

One last word—Behaviorism, as I said before, is not necessarily Mechanical. Though most behaviorists profess adhesion to the mechanistic dogma, they are even now making use of terms, such as 'motor sets,' 'trends,' 'drives,' and 'determining tendencies,' all of which are terms of the Purposive Psychology, thinly disguised. I have no doubt that Dr. Watson is capable of giving you the psychology you need and that he is developing for you a Purposive Psychology. Like Münsterberg, and like myself, his first phase of psychology was in the purely academic atmosphere which hitherto has been that of the Mechanical Psychology. But he has now for some years been active in the sphere of Applied Psychology; that is to say he has passed into, perhaps through, the second phase through which Münsterberg and I myself have passed. I have good hope that he will come out, perhaps already has come out, on the right side. If then you must be behaviorists, I beg that you will be purposive behaviorists.

ON THE POSSIBILITY OF AN APPLIED PSYCHOLOGY¹

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I. INTRODUCTION

It is startling to realize that successive generations in the family of the sciences are born and reach maturity sometimes in years as few as those which measure the generations of the human family. Psychology as an experimental science was born almost within the lifetime of the writer; and he remembers well when in many of our universities every psychologist was supposed to be trained and competent as a philosopher also, and *vice versa*. The year 1879, when Wundt opened in Leipsic the first psychological laboratory, is often designated as the birth year of experimental psychology; so the splitting off in 1902 of the American Philosophical Association from its parent society, the American Psychological Association, may be taken as the year in which the science reached adulthood. For here was formal recognition that philosopher and psychologist no longer had to be one and the same person.

Today we are witnessing the growth of a lusty grandchild in this family: psychotechnology, offspring from the union of psychology and practical affairs. This child too will soon reach a healthy maturity, ready to go its own way. There is noticeable even now a tendency to functionalize the profession, to differentiate responsibility for making psychology useful from responsibility for advancing, through teaching and research, the pure science itself. Such functionalization has long been employed in other sciences. As engineering is to physics, as scientific agriculture is to biology, as medicine is to physiology, as pharmacology is to physiological chemistry, as forestry is to botany, as statistics is to mathematics, as

¹ Read in part at the Cambridge meeting of the American Psychological Association, December 28, 1922.

navigation is to astronomy, so ought psychotechnology to be related to psychology.

Already a few trained men are earning a livelihood as practicing psychologists. To be sure, most of the work of commercial application is still done as an avocation, by psychologists who are first of all scientists rather than technicians and often of necessity but slightly familiar with the setting of the practical problems to be solved. Even so, the achievements in applied psychology have been measurable. Many are the distinguished scientists who have helped its development. In the roster of past presidents of the American Psychological Association are few who have not engaged in the practice of some phase of applied psychology and made additions to the substance of its subject-matter. Being a psychologist has meant being both scientist and human engineer.

Up to the present time, differentiation or functionalization has not proceeded far. It has been assumed that any real psychologist could apply his science—insofar as it had possibilities of practical utility—and that any one who was competent in psychotechnology must be a psychologist. But today this identification is being questioned, just as twenty-five years ago the question was raised whether every psychologist must be a philosopher. There has been set up in the American Psychological Association a section for practicing psychologists, with standards of eligibility which not all psychologists can meet. Tomorrow the young discipline of applied psychology, with its own journals, its own specialists, its own body of scientifically verified content, and its own technique, may be amply able independently to pursue its own proper objectives—the useful prediction and control of human conduct.

And yet, some highly able psychologists like to say—at least in private conversation—that there really is no such thing as applied psychology; and the outstanding accomplishments of psychologists in education, industry, and war have not served completely to silence these remarks. For, what is taught in our universities under the title of applied psychology, they say, is not really a technology in any strict

sense of that term. Instead, it consists only of excerpts from pure psychology, padded with illustrations, speculations, and hopes as to how these fragments of principle or method might be pertinent to practical problems. They challenge applied psychology to show any worthwhile subject-matter which is not clearly the property of general psychology or else merely a precipitation of common sense accumulated by workers in the different special fields of application, such as law, medicine, education, and business. There exists today we are told, no independent discipline of applied psychology, no psychotechnology worthy the name. We are invited to ask whether there is the possibility of an applied psychology, a technological discipline whose regulative concepts can be stated in a way which differentiates it usefully from general psychology. If so, of what sort is its content? And what are its differentiae and its proper aims?

II. EXAMPLES OF CONTENT

When challenged to exhibit specimens of content in applied psychology—useful technique or principles that can be taught but that are not the common possession of psychologists as such—the answer may be hesitant, but only because of the necessity of selection from among a multitude of illustrations.

Anyone who has tested children or mentally disordered persons knows that psychometric procedure, throughout its whole range, is so dotted with bits of specific skill and technique that inexperience inevitably leads to blunders.

Let me here cite a few generalizations regarding psychometric practice, to which we have been led at Carnegie Institute of Technology, first letting fall the caution that these statements apply only to measurements of normal adult mentality.

1. When it is necessary to resort to subjective estimates or ratings, more reliable results are obtainable by aid of well constructed *graphic* rating scales than by any of the earlier forms of rating scales.

2. Group tests of intelligence are more useful than individual tests for purposes of an initial inventory.

3. Time-limit methods yield better measures than amount-limit methods.

4. Omnibus tests are superior to separate tests for practical service, but not for research purposes.

5. In measuring amount of information or degree of skill, stencil examinations—that is, objective test questions calling for answers that are definitely either right or wrong—are superior to questions where the answer contains an element of value to be estimated by the examiner.

6. But the stencil examination does not measure creative powers.

7. Intelligence tests are less predictive of success in social occupations, such as selling, than are interest analyses.

8. In predicting probable success in many types of positions, it is essential to determine an upper critical score, as well as a lower limit on the intelligence scale.

Every one of these statements may strike the eye as more or less obvious. But not one of them was known in 1915. Then, dozens of psychologists were wasting time devising new tests and using the closeness of approximation to a symmetrical curve of distribution as the only indicator of validity. Or, high correlation of the new test scores with scores in some other examination such as the Binet, was taken as a favorable index. Today few investigators in the testing field would ever experiment with a new test without first getting some dependable *objective criterion* of its validity.

A related province of applied psychology in which we have recently accumulated some items of practical technique is in the making of job analyses. See Strong and Uhrbrock's new volume on job analysis of printing executives.¹ Also, in making job analyses of clerical positions in large offices, Yoakum, Miss Bills, and other members of the staff of our Bureau of Personnel Research have arrived at certain practical rules of procedure. For instance:

I. Make the inventory of duties yourself. Do not entrust

¹ Strong, E. K. Jr., and Uhrbrock, R. S., 'Job Analysis and the Curriculum: with Special Reference to the Training of Printing Executives, *Personnel Research Series*, 1923, 1, 1-146.

it to inexperienced analyzers in the organization being surveyed.

2. Begin with the lowest grade of jobs and work up. (This is the reverse of Scott Company practice, but we are prepared to defend our procedure.)

3. Get every item down in black and white, and get it approved in writing by the worker himself and by his immediate superior. In making the job classification later, use no information regarding duties and responsibilities that is not so recorded and O.K.'d.

4. In classifying clerical jobs for purposes of wage adjustment, about nine levels of skill and responsibility can be clearly differentiated.

5. To avoid antagonism of the workers, such as Scientific Management aroused, an employer should not make radical adjustments downward of wages of individuals whom the clerical survey shows to be getting relatively too much pay. Employees who are found to be underpaid should have their wages raised, leaving it to time and turnover gradually to eliminate those who are now overpaid.

From a contiguous field of research may be chosen a single illustration, of value to anyone planning an investigation on data to be gathered in personal interview. Charters¹ has found, and repeatedly verified the fact, that in gathering specific items of difficulties, duties, or methods of procedure in a particular job, the number of workers in that job whom it pays to interview is about thirty. If the interviewer stops short of twenty-five, he may be certain of missing several specific points. If he goes beyond thirty, little if any new information is obtainable. Ninety-seven per cent of the items are obtained in the first thirty answers to each question.

These are but sample illustrations of principles of procedure, distilled in part from practical experience as well as from scientific experiment. Examples might similarly be gleaned from the applied psychology of acting, writing, preaching, teaching, selling, advertising, medicine, or social

¹ Charters, W. W., 'The Collecting of Unrecorded Specifics,' *J. of Educ. Res.*, 1922, 5, 280-294.

work. From such accumulations of technique is now being built the growing structure of applied psychology.

III. CONFUSIONS AS TO THE AIM OF PSYCHOLOGY

Applied psychology in the broadest sense is *psychology in the service of ends other than its own*. This definition would furnish an excellent starting point if there were no uncertainty as to the proper aims of psychology itself. Unfortunately a survey of recent literature leaves the inquirer in doubt as to whether our aim as psychologists is primarily scientific, or practical, or both.

Doubtless some of these ambiguities of aim are due to the rapidity with which practical psychology has recently expanded. As a matter of fact, a youthful psychotechnology has already been infiltrated into the science of psychology without our full awareness of what has been happening. Everyone has indeed seen the revolution in psychological activity wrought by the demands of war, of industry, of education, of medicine, and of social work. Every one has noted the multiplication of researches on practical problems, the increase in the number of psychologists engaged in testing and in personnel administration, the encroachment of courses in applied psychology on student time formerly held inviolate for experimental and systematic training. But not everyone has noticed the resulting vagueness and confusion in the minds of many psychologists as to what psychology really is.

Demands of undergraduate classrooms have tended to heighten the confusion. Elementary students are fond of hearing about the psychology of advertising and selling, eugenics, labor mediation, the typography of the telephone book, or stereoscopic vision in the movies; and many instructors have tended more and more to fill their introductory lectures with such fascinating topics of psychotechnology. Some teachers have gone the length of introducing students to psychology through the technological gateway, beginning their courses with laboratory exercises in measuring the strength of appeal of advertisements, or the relative effectiveness of methods of learning. No one should deplore

such an inductive and concrete approach, provided the instructor keeps always before his own thought the distinction between pure and applied psychology, and insists eventually on a clear recognition by his pupils of this disparateness of aim and content. Unfortunately, this is not always done. Some psychologists seem to have forgotten for the moment that psychology is a science apart from its applications; or they have been led by the practical demand mistakenly to regard psychology as at once a science and a technology.

There is, to be sure, a large company of traditional psychologists who continue faithfully to construe psychology as a natural science and who define its aims to be the *description and explanation* of the phenomena it studies. When the facts of human nature are adequately observed, analyzed, classified, described, and explained, these psychologists construe their task as finished. Such a goal does not require an extension of responsibilities into the realm of practical application. Meanwhile, other psychologists keep before themselves practical aims of controlling human conduct as their ultimate. They feel that their task is not finished until they have forged the metal of their science into working tools.

No clearer statement of this position could be desired than that of McDougall on the first page of his recent 'Prolegomena to Psychology': "The aim of psychology is to render our knowledge of human nature more exact and more systematic in order that we may control ourselves more wisely and influence our fellowmen more effectively."¹ Psychology, according to such a view, arrives at its goal only when it has contributed to the effectiveness of control over ourselves or our fellows, either directly or through contributions made to the social sciences. Pure psychology is, then, for McDougall, not an end but a means.

Watson is another writer who aims at a technology as well as a science. It should be noted, however, that a Watsonian behaviorism as such has no monopoly on practical psychology. For example, Dunlap's latest text-book says: "The psychology of today is a science of the conscious re-

¹ McDougall, W., 'Prolegomena to Psychology,' PSYCHOL. REV., 1922, 29, 1-44.

sponses of the organism, and as such is called upon to furnish materials applicable to the problems of physical science, education, industry and the arts; and to social problems."¹

Contributions to the technique of prediction and control of conduct have been made by introspectionists, as well as behaviorists; while some behaviorists seem content to study behavior as a descriptive science, indifferent to the practical values to which their findings may or may not lead. That practical psychology and behavioristic psychology have tended to be identified is, then, more or less of an accident. If it should be decided that psychology studies behavior only, or experience only, or both, the question still arises whether this subject-matter is studied for scientific purposes of description and explanation, or for practical purposes of prediction and control. No matter whether introspection, or objective measurement, or both methods, are finally decreed to be valid and authentic, the query must nevertheless be met, whether these methods are employed in the service of psychology the science, or in the service of more remote and practical ends. Progress in the science itself and in its applications will, I believe, be fostered if all schools of doctrine recognize that there are these two sharply divergent objectives, the scientific and the technological.

The lines of demarcation seem so obvious that one might apologize for pointing to them, were it not for the current disagreements of attitude and practice. Pillsbury, for example, implies a practical aim but writes a volume of substantial science almost devoid of more than casual reference to utilities. Hunter announces a position diametrically opposed to that of this paper when he says that the term applied psychology "makes an unwarranted division between the psychology of an individual and the applications of this knowledge to some practical end."² But it is likely that he has in mind the use of the term as a synonym for individual psychology and is overlooking the rapidly growing accumulations of practical principles and technique that have

¹ Dunlap, K., 'Elements of Scientific Psychology,' 1922, p. 7.

² Hunter, W. S., 'General Psychology,' 1919, p. 53.

been erected on the foundations of individual psychology. Warren, on the other hand, makes a sharp differentiation. "Applied psychology," he says, "is not a division of psychology...; it means the art of using in practical ways the results obtained from psychology."² The writers of psychological textbooks are not agreed as to the definition and aims either of psychology or of applied psychology.

Cattell in founding the Psychological Corporation finds no need for the term 'applied psychology,' although it may be predicted that the success of this undertaking will hasten the coming of an adequate psychotechnology. Psychology is for Cattell a blanket term. Indeed, he once defined psychology as 'what a psychologist works at.' But that was twenty years ago, when our science still enjoyed Youth's license to be impudent. In our maturer years we can hardly continue, as psychologists, to work at optics, neurophysiology, advertising, mental therapy, and statistical theory, without apology to disciplines on whose bounds we are frankly poaching. Indeed, some are apprehensive lest psychologists devote their efforts exclusively to surrounding fields, forgetting to cultivate the scientific study of experience—the proper zone of psychological research. They utter forebodings of the day when we shall discover ourselves expatriated, aliens in territories obviously owing no allegiance to any psychological banner. These apprehensions need hardly be taken seriously, because psychologists will, in due time and measure, give thought afresh to the real nature and aims of their own science. But meanwhile, it is no wonder that divergencies of leadership and aim have permitted some of the younger psychologists to flounder. borne along on the huge wave of mental test activity, forgetting to steer straight for scientific objectives and aiming at what appeared to be the immediately practical, some of them have sought quick rewards of personal prominence or tempting consulting fees or royalties on new half-standardized test blanks, until output has inevitably failed at times to meet the requirements of either science or technology. These must consider anew the objectives, the ideals, the standards, of psychology both pure and applied.

² Warren, H. C., 'Elements of Human Psychology,' p. 15.

The proper aim of psychology as a science is similar to that of all natural science, namely, the understanding of the phenomena it studies irrespective of whether this knowledge does or does not bear on any practical problem of control. Over against this goal, the realization of which brings knowledge, stands the technological objective. A technology aims not at the understanding but at the control of the phenomena it studies. Or, when it seems to aim at understanding, it is for the ultimate purpose of control. As description and explanation is the goal of science, the goal of technology is prediction and control.

If this contrast is overlooked and psychology defined as really not being psychology until it has been made to yield a technique of control, then of course there can be no independent discipline called applied psychology, because no line of demarcation remains. But with a clean-cut statement of the aim of scientific psychology which rests content with intellectual explanations of experience or behavior, we can profitably draw the line between psychology the science, and applied psychology, which is a technology.

There is, then, logically, the possibility of a pure psychology apart from an applied psychology. When an investigator's efforts are focused toward an enlargement of our knowledge of human nature, he is pursuing the ends of a scientific psychology. But when his goal is frankly the prediction and control of behavior for practical ends, he is aiming toward a psychotechnology. The clear recognition of these two contrasted aims, the scientific and the practical, cannot fail to benefit both the pure and the applied science. It will encourage a functionalization of the psychologists' work. It will permit a division of labor, a concentration of effort, a specialization which is becoming increasingly necessary as our discipline grows in accomplishment and in bulk of subject-matter.

IV. APPLIED PSYCHOLOGY EMPHASIZES THE TECHNIQUE OF CONTROL

Some readers will object that this definition of the aim of pure science—limiting it to description and explanation—is

too narrow. He who really understands, they will say, already has in his possession the reins of mastery. Knowledge is power. Explanation, as the goal of pure research, is not yet complete if it leaves anything to be desired in the way of intellectual grasp of causes and effects which warrants accurate prediction and permits full control of the phenomena studied. When explanation of behavior is complete, will pure psychology not already have provided that control of behavior which is the goal of psychotechnology?

In answer, let it be recalled that skill and technique are often as essential for control as is knowledge or understanding. To have the system and routine of a great office explained in full does not provide one forthwith with the ability to administer it smoothly. To know everything that has been published about the Stanford-Binet examinations, including all the precautions regarding the administering of each test, does not guarantee that an inexperienced psychologist will not find himself hampered by embarrassment before the child to be examined. Control is a matter of experience and technique no less than of scientific knowledge. Here then is a second basic difference between pure and applied psychology. The applied science must emphasize the technique of control. It aims at a mastery of practical circumstances and details as well as of general principles of explanation. To be sure, applied psychology must lean heavily on workers in pure science for discovery of explanations; but it must add to these explanatory principles a treatment of the means of employing them with proficiency and effectiveness.

It is helpful to keep in mind four kinds of control: intuitive, empirical, intellectual, and technological.

Some men are intuitively much better judges of human nature than others. A superior executive makes fewer mistakes in choosing his subordinates. He may not be able to tell how he does it. He may not be able to pass on to anyone else his ability in picking men. His predictions are outgrowths of his own experience and native insight. They are intuitive. A mother knows exactly what to do to quiet her fretful child. A certain teacher always commands respect and order in his

class-room. A skillful salesman knows just when to try to close. A good foreman has his own effective procedures for getting a full measure of work and of loyalty from his gang. These are instances of intuitive control if the person himself does not know *how* the thing is done. If he knows how he does it, the control is probably rule-of-thumb, or what we may call empirical control.

If he also knows *why* the procedures work, if he has an understanding of the problem and a grasp of the causal factors that determine the desired responses, he has an intellectual control of behavior. It is this third type of control which science can provide.

But in the realm of practical affairs it is, as we have seen, not enough only to understand. To scientific insight must be added for successful action a measure of necessary technique or skill. A technology of human behavior includes a treatment of this needed skill and of practical procedures. The second, third, and fourth types of control are each necessary: rule-of-thumb, rational understanding, and technique—all must be included in the subject-matter of technology. Only the intuitive methods of control are excluded.

In this paper, the terms 'prediction' and 'control' are used in this broad technological sense. So interpreted, they are useful concepts in the differentiation of psychology from psychotechnology.

An applied science, then, is at once narrower and broader than its allied pure science. While omitting facts and principles that have not been found to have utility values, the applied science includes within its scope much systematized knowledge about skills, techniques, and aids to successful performance regarding which the pure science is silent. Moreover, a technology cannot despise an empirical formula or well-tried rule-of-thumb, where need is pressing and science fails. Applied psychology differs from pure psychology in the prominence it gives to the technique of control, as well as in its practical aim.

V. PRACTICAL STANDARDS

The nature of the aim of applied psychology establishes certain standards that must be used in the selection of methods and procedures. The standards a technology uses must be pragmatic ones: Does this principle, formula, or procedure work? Is it practically helpful under the complex conditions that prevail in factories, schools, and hospitals? Is it an aid, within the narrow range of immediate need where it is employed? Does it earn more than it costs? The criteria of pure science, on the other hand, demand consistency within a larger field of knowledge, but give no heed to questions of cash value or to other narrowly practical considerations.

In industrial and commercial psychology particularly, as has been pointed out by Rumel, the choice of methods and procedures must rest, in the last analysis, not only upon accuracy but upon costs; "or more truly upon whether the direct and indirect return in dollars, after deducting all costs involved in the use of the technique, is greater from one method than it is from another. The psychologist working in industry is vitally interested in accuracy of method; but his interest is in whether this accuracy costs more than it earns."¹ This emphatically does not mean that the practical methods of industrial work are therefore crude in comparison with the exact technique of the laboratory. Says Rumel, "The comparison of laboratory with practical procedure rests on something more fundamental and something quite different from the contrast between exact methods and rough, between the precise and the approximate. The elements of quantitative exactness, of accurate insight, of imagination, are present in both cases. The virtues and vices in alternative procedures must be studied. It is the criterion of selection of method that differs.

"A comparison of laboratory methods with practical methods that implies for one exactness and for the other looseness, that suggests for one precision and for the other approximation is essentially unsound and misleading. It

¹ In an unpublished paper read before the Section of Anthropology and Psychology of the New York Academy of Sciences.

blinds the worker in applied psychology to the significant point of his study: it breaks his scientific morale. The methods of applied industrial psychology are, or at least certainly ought to be, chosen with as much discrimination as are the methods of the laboratory, after a study of all the factors influencing choice of method that are pertinent to laboratory investigation—and one factor in addition, the dollar return.

"The psychologist who takes up the problems of industry must also recognize the criteria of industry. The merit of his work, even from the standpoint of science, is judged by his resourcefulness in detecting and eliminating costs and by his far-sightedness in securing and estimating returns."

VI. THE BASES OF TECHNOLOGY: SCIENCE AND EXPERIENCE

It is a common error to think of technology as always resting securely on a broad basis of pure science. We forget that historically the opposite is the case. Practice has preceded theory more often than theory has pointed the way to practice. Sturdy stone bridges were designed and built before ever a physicist formulated a general proposition about the law of gravitation or the parallelogram of forces. Many a strong stick was used as a crowbar before Archimedes enunciated the principle of the lever. Even today, after a century of marvellous scientific progress, industrial practice is in many instances far in advance of scientific theory. The Cottrell process of extracting ores by flotation has recovered millions of value from otherwise worthless dump heaps, although the most advanced students of physical chemistry cannot yet explain the behavior of the colloid solutions that do the work. The mechanical engineer's hand-book contains numerous empirical formulæ and tables which he uses in design with entire confidence, although the physicist has not yet discovered the principles which explain in detail why the properties of the materials in question behave as they do. The establishment of procedures valuable in industry often far outruns the ability of theory to furnish a scientific explanation. Technology and science are not rigidly yoked to-

gether. Technology leads the way and sets the problems for science, as often as it builds upon and utilizes pure science.

To this rule psychotechnology is no exception. The skillful and thoughtful executive, the statesman, the physician, the salesman, the parent, the teacher, the warrior, have all been accumulating and practicing applied psychology since the dawn of human society. Some of the psychologically soundest principles and precepts in Scott's 'Psychology of Public Speaking' are found in 'The Training of the Orator,' written by Quintilian in the first century. The development and utilization of mental tests has outrun the theory of intelligence. The practical discipline will in its future progress be expected always to lean heavily on pure psychology; and often, let us hope, it will be able to add riches to the store of pure psychology. Advances in pure research will often have important unforeseen practical bearings, as has frequently been the case with researches in the physical sciences. Similarly, discoveries of practical psychology will sometimes yield material significant for theory, just as mechanical engineering has frequently discovered in the course of its practical researches, new general truths about the physical properties of matter. Like engineering, applied psychology must systematize and extend its knowledge in detail, far beyond the scope of the pure science; and must develop adequate and dependable empirical techniques where need outruns the advance of general theory.

Functionalist disciples of James and Angell may here raise the point that practical utilities are included in the proper subject-matter of pure psychology because a mental process or an act is not explained until its utility is understood. But even this position would scarcely warrant pure psychology in trying to swallow up applied psychology, since an explanation of the biological function of a response is quite other than the technique of its control. The position may even be maintained by the functionalist that all thinking arises out of a felt need and has some practical issue as its goal; that the intellect is a supreme tool which biological evolution has developed for grappling with refractory cir-

cumstance. Nevertheless, it must be recalled that one of the 'practical' needs of life is the satisfaction of that profound instinctive trait of human nature, curiosity; that many a scientist has a passion for orderliness, for system, for comprehensiveness of knowledge within his chosen domain; and that consequently the pursuit of pure science need not seek extraneously for its justification or its sufficient motive. The scientist, as such, does not require the spur of the narrowly practical. The advancement of his science is an adequate drive. Contributions to pure science may or may not lead ultimately to practical consequences other than the satisfaction of the craving for knowledge. Even if one accepts a functional statement of the nature and the place of mentality, the contrast between pure and applied science remains valid and useful.

There is, of course, in spite of gross differences, much that is identical in any technology and its basic science. Many principles, facts, and methods they have in common. The radical difference after all is one of aim. The scientist seeks knowledge; the technician, control. The pure science disregards considerations of cost; the applied science must always weigh the practical values of every achievement against the necessary expenditures of money and effort.

Applied psychology will one day be practiced and written and taught around certain fundamental and well recognized functional concepts, chief of which are the concepts of practical utility and of control: control of behavior in one's self and in others; control of individuals and control of societies; control of behavior of persons who are in sympathy with our purpose, who are opposed to our purpose, or who are unaware of our purpose; temporary control of immediate responses, and permanent modifications of reaction patterns. Such a discipline will have a sound basis of organization. It will develop those general principles, methods, and results which place in the learner's hands the tools for effecting the control of behavior; and these principles will be made concrete through illustrations drawn freely from the many separate vocations which these functions must serve. Thus equipped,

the student can then pursue further his inquiries into the literature of any special field; or better still, go forth to face practical problems as yet unsolved, knowing that he understands the basic principles of control, and has some practice in the methods of investigation into the problems that arise in the utilization of these principles.

It has been the purpose of this paper to call freshly to mind just what are after all the legitimate aims of any science and of psychology in particular; and to raise the question whether the time has not come to direct psychology anew toward its proper goal of scientific achievement by separating from its primary tasks the burden of application.

Summary.—This paper sets forth a warrant for the existence—or at least, the possibility—of an applied psychology. The contrasts between a pure science and a technological discipline are drawn in order to bring into relief the regulative conceptions of an applied psychology. Pure psychology completes its task with description and explanation of the phenomena it studies. The functions of prediction and control for practical ends lie outside of its scope, and are the proper subject-matter of psychotechnology. Some examples of content are cited. Confusions incident to an erroneous identification of behaviorism with practical psychology are indicated, as are also vaguenesses traceable to a prevalent tendency to define ambiguously the aims of psychology as at once scientific and practical. A clear recognition of these differentiae between psychology the pure science and psychotechnology will have beneficial consequences in the conduct of research, in the editorial policies of the psychological journals, and in the organization of instruction.

THE EARLY DEVELOPMENT OF HARTLEY'S DOCTRINE OF ASSOCIATION

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I

Samuel Parr's 'Metaphysical Tracts by English Philosophers of the Eighteenth Century' is well known as one of the most interesting and valuable compilations in British philosophy. The compiler died in 1823 and this work did not appear until 1837, hence it is a posthumous publication. Although the works published by Parr during his lifetime are chiefly of a political and theological nature yet he was regarded by his contemporaries as somewhat of an authority in metaphysics owing to his wide information. In 1820 he was consulted by Sir William Hamilton in reference to a suitable successor to Thomas Brown as professor of moral philosophy in Edinburgh University. He was a correspondent of Dugald Stewart to whom he supplied considerable data for his works. Indeed Parr's essay on the word 'sublime' which he sent to Stewart, if printed in full instead of as an abstract,¹ would have filled 250 pages of that philosopher's works. Parr was also a friend of Priestley at a time (1791) when such friendship was not unaccompanied with danger. Bentham too visited him in 1803 and received a mourning ring at his death.

Parr had a particular admiration for the writings of Hume, Hartley, Hutcheson, and Adam Smith. He attempted to read Kant 'but gave him up as a bad job' as French was the only modern foreign language with which he was familiar. He was himself largely in agreement with the utilitarian position. In the philosophical realm, however, it is because of the collection of 'Metaphysical Tracts' that the name of Parr will be chiefly remembered.

The tracts included by Parr in his collection are five in

¹ Stewart's 'Works,' V., pp. 455-485.

number. The first one has the title of 'Clavis Universalis, or a New Inquiry after Truth. Being a Demonstration of the Non-existence of an External World' by Arthur Collier. It bears the date of London, 1713. The second tract is also by Arthur Collier and is entitled 'A Specimen of True Philosophy, in a Discourse on Genesis, the first chapter and first verse.' It was first printed in Sarum in 1730, and on the title page it is said that it is 'not improper to be bound up with his Clavis Universalis.' The third tract, by David Hartley, will be discussed presently. The fourth is anonymous and is entitled 'An Enquiry into the Origin of the Human Appetites and Affections, shewing how each arises from Association.' It was printed at Lincoln in 1746. The fifth and concluding tract is by Cuthbert Comment (a pseudonym for Abraham Tucker). Its title reads: "Man in quest of himself; or a Defence of the Individuality of the Human Mind or Self, occasioned by some remarks in the Monthly Review for July, 1763, on a note in Search's 'Freewill.'" It was dated London, 1763.

Of these tracts by far the most important is the third, written in Latin by David Hartley and entitled 'Conjecturae Quaedam de Sensu, Motu et Idearum Generatione.' It bears no date. It is believed that the discovery of the actual time of its publication as here attempted will throw new light on the early history of Hartley's development of the doctrine of association.

II

From whatever source Parr may have obtained this Latin tract it is certain that the correct date of it was not known to him. He believed it to have been printed in 1748. This fact is learned from a note in the Dissertations of Dugald Stewart who was much puzzled by the date accepted by Parr. "In a letter," writes Stewart, "which I received from Dr. Parr he mentions a treatise of Dr. Hartley's which appeared about a year before the publication of the great work [in 1749] to which it was meant by the author to serve as a precursor. Of this rare treatise I had never before heard." "You will be astonished to hear," says Dr. Parr, "that in this book,

instead of the doctrine of necessity Hartley openly declares for the indifference of the will as maintained by Archbishop King." We are told by Hartley himself that his notions upon necessity grew upon him while he was writing his 'Observations on Man'; but it is curious (as Dr. Parr remarks) that in the course of a year his opinions on so very essential a point should have undergone so complete a change.¹ In another note of the *Dissertations restored* in Sir Wm. Hamilton's edition of Stewart's works he also writes: "Of this first work of Hartley's as previously stated, I had never heard before; and from the manner in which Dr. Parr writes of it, I presume it is very little known even in England (June 1820). I am glad to add that a republication of it, and of some other rare tracts on metaphysical subjects may soon be expected from this illustrious scholar and philosopher. Among these tracts it gives me particular pleasure to mention 'Clavis Universalis' of which I had occasion to take notice in speaking of the Idealism of Berkeley."²

As the date of publication of Hartley's 'Observations on Man' was 1749, that of the Latin tract, if it had appeared as Parr believed in the previous year, would be 1748. Moreover if the tract contained a statement in favor of 'the indifference of the will,' Hartley's complete change of attitude in one year, had it taken place, would certainly as Stewart writes be 'curious.' But it is even more strange that Stewart did not investigate the truth of Parr's conjectures instead of filling his footnotes with remarks upon the erroneous data of the compiler of the tracts.

The confusion of writers in regard to the date of the appearance of this Latin tract does not cease with Stewart. Edward Tagart published a work in London in 1855 on 'Locke's Writings and Philosophy Historically Considered,' in which he also gives an outline of the progress of English philosophy. In his 'Defence of Hartley' he adds a footnote

¹ 'Collected Works of Dugald Stewart,' ed. by Sir Wm. Hamilton, Vol. I., *Dissertations, Part II.* (1821), note, p. 355.

² Sir William Hamilton remarks upon this restored note in his edition of Stewart's 'Works': "I may also mention, that the collection here referred to, and which was printed previously to Dr. Parr's death, has since been published by Mr. Lumley."

(pp. 150-1) upon 'This precious volume of tracts' prepared for the press by Dr. Parr, in which he quotes from Stewart the passage already given in regard to the Latin tract of Hartley as brought to his notice by Parr.

"Dr. Parr's remark," as Tagart writes, "does not quite amount to Mr. Stewart's interpretation of it; but I believe the whole passage is without foundation, and that the truly candid and kindhearted Parr was for once mistaken. Hartley was employed eighteen years upon his work, and his opinions were not likely to change suddenly and without deep thought upon such a subject. The Latin tract is a short but excellent abstract of the great work; and of many of the most interesting passages, particularly of an historical character, it is strictly a translation. I have not found the phrase 'Liberum arbitrium' in it. In the 'Scholium Generale' where Hartley sums up the doctrines of vibrations and association, nothing is said about freewill or necessity."

Here then is an exactly opposite view to that held by Parr. Whereas Parr believed the Latin tract of Hartley to have preceded by a year the publication of the 'Observations on Man,' Tagart thought the tract to have been subsequent in its appearance as implied in the statement that it was an abstract and of many passages even a translation of the great work. What then is the truth?

If we turn to the opening sentence of Hartley's Latin tract, when translated it reads as follows: "May it be permitted to add something to the dissertation on Lithontriptics concerning sense, motion and ideas; which although not closely connected therewith nevertheless pertains to the general theory of medicine and may be of service in advancing it." Hartley here affirms the Latin tract to have been written as a supplement to a medical work on Lithontriptics.

It is known that Hartley was a physician and the author of various medical productions. He was, moreover, a sufferer from the disease of the stone that is mentioned in the title of this medical dissertation. It is also certain that he had a firm belief in the remedy for the stone announced by one Mrs. Stephens, as he wrote an article in her defence and helped to

secure for her in 1739 a grant of £5000 by parliament for the publication of her secret. In this way it came about that in 1741 there also appeared from the pen of Hartley a dissertation entitled: 'De Lithontriptics a Joanna Stephens nuper in vento epistolarum' (Leyden, 1741). And it is to this particular medical dissertation that he thus proposes in the opening sentence of the Latin tract to write a supplement concerning sense, motion, and ideas.

This account of the production of the Latin tract is however here not yet complete, as its connection with the dissertation on Lithontriptics is closer than has thus far been intimated. The Latin tract is not only a supplement to the dissertation on the stone, but first appeared bound up with that work rather than as a separate publication. In the year 1746 there was published by Hartley at Bath a second edition of his 'De Lithontriptics a Joanna Stephens, etc.' to which was added in the same volume the identical Latin tract that was afterwards included by Parr in his 'Metaphysical Tracts.' The full title of the second edition of this medical work also bears out this statement, as it reads: "De Lithontriptics Joanna Stephens nuper invento dissertatis epistolarum (Lugd. Bat. 1741) 2 ed. cui adjicitus methodus exhibendi lithontripticum sub forma commodiore . . . accedunt etiam conjectuae quaedam de sensu, motu et idearum generatione." Bathoniae, 1746.

We thus discover that neither Dr. Parr nor Mr. Tagart were correct in their beliefs as to the date of publication of Hartley's Latin tract. It did not appear as Parr thought in 1748, one year before the publication of the 'Observations on Man' but antedated that great work by three years. This lengthened period would have given of course greater opportunity for a change of belief on the subject of necessity by Parr which so worried Stewart, if it were correct (which it is not) to assume that any such had taken place. On the other hand, the Latin tract having appeared, as already shown, in 1746, was not as Tagart thought either an abstract or a translation, but as Stewart correctly says 'the precursor' of Hartley's 'Observations on Man' since the latter work was

not published until 1749. While neither Parr nor Tagart were correct in regard to the date of the appearance of the Latin tract, the former was nearer the truth than the latter, since he placed it before instead of after the publication of the 'Observations on Man.' On the other hand, we later discover that Tagart rather than Parr possessed the truth in regard to the entire absence of any change of mind upon the question of freedom on the part of Hartley between the date of publication of the Latin tract and of the later appearing 'Observations on Man.'

III

Having thus ascertained that 1746 was the correct date of the appearance of Hartley's Latin tract which was reprinted by Parr, it is now possible to investigate the more important question of the light thereby thrown upon the course of the development by him of the doctrine of association. "About eighteen years ago" (*i.e.* 1731), Hartley writes in the preface to his 'Observations on Man,' "I was informed that the Rev. Mr. Gay, then living, asserted the possibility of deducing all our intellectual pleasures and pains from association. This put me upon considering the power of association. Mr. Gay published his sentiments on this matter about the same time [1731], in a Dissertation on the fundamental principle of virtue, prefixed to Mr. Archdeacon Law's translation of Archbishop King's 'Origin of Evil.' From inquiring into the power of association I was led to examine both its consequences in respect of morality and religion, and its physical cause. By degrees many disquisitions foreign to the doctrine of association, or at least not immediately connected with it, intermixed themselves. I have here put together all my separate papers on these subjects, digesting them in such order as they seemed naturally to suggest; and adding such things as were necessary to make the whole appear more complete and systematical." The doctrine of association was therefore the subject of reflection and development by Hartley for the period of eighteen years, from 1731 when he received the first suggestion of its possibilities from

Gay to 1749 when he published the final results of his thoughts in his 'Observations on Man.'

During this period of reflection on the doctrine of association by Hartley, it appears from his own words that a number of disquisitions were written by him entirely foreign to that subject but others were more or less connected therewith. On December 12, 1736, he writes to a Mr. Lister that he had finished two small treatises about a year and a half ago, called 'The Progress of Happiness Deduced from Reason' and starting from the principle of association. The disquisition here mentioned as based on association was therefore written early in the year 1735. "In 1738," as we learn from the article on Hartley in the National Biography, "he had enlarged his plan and contemplated an 'Introduction to the History of Man' in four parts. He sent rough drafts of the first two parts to Lister in that year, and afterwards replied to Lister's criticisms defending his own doctrine of determinism and universal happiness and condemning Butler's doctrine of resentment." Inasmuch as Hartley was here strongly maintaining the doctrine of determinism as early as 1738, we perceive how groundless was the agitation of Dugald Stewart occasioned by Parr as to the sudden adoption of that doctrine as late as the year 1748, or one year previous to the publication of the 'Observations on Man.'

During the years 1738, 1739, and 1741 Hartley wrote the three disquisitions which were foreign to the doctrine of association. These related as we have seen to the disease of the stone and the remedy for it of Mrs. Stephens. And it was to the last of these dissertations, that on Lithontriptics of 1741, that he added in the second edition of 1746 the Latin tract 'Conjecturae quaedam de sensu, motu et idearum generatione.' This addition was made, as he remarks in the introduction to the tract, because although the sense, motion, and ideas are not closely connected with Lithontriptics they nevertheless pertain to the general theory of medicine and might prove of value to it. "For," he continues, "they are deduced from this theory in the shape in which it has now been elaborated by anatomists and physicians, taken in conjunc-

tion with what Newton has written in regard to vibrations transmitted through the brain of animals, and what Locke and other famous men since his time have written in regard to the power of association upon the human mind. Relying myself on such eminent assistance and authority I in time past undertook the further elucidation of sensations, motions, and ideas, and I think that I have at last succeeded in arriving at some sort of truth. As many objections still remain I have decided to outline a conjectural theory of these things, and taking advantage of this opportunity to subject it to the free examination of medicine and of philosophy, so that I then may be taught what ought to be corrected, removed or retained. I have put forward my conjectures, however rude and uncertain, under the form of mathematical demonstrations, inasmuch as this seems the most convenient form to arrive at the force and meaning of things and of association."

The two principal theories of Hartley's Latin tract are doubtless prompted in large measure by the twofold interests of the man as a physician and as a philosopher. His researches as a physician and a physicist would lead to the theory of vibrations by which he explains all nervous phenomena and the relations of body and mind. His reflections as a philosopher and a psychologist would likewise account for a theory of association by which he explains the entire mechanism of the mind. Although the two theories may have been independently suggested yet they naturally became intimately connected and interwoven in the course of their development. In this way the Latin tract combined elements both of physiology and psychology. Hartley may therefore very properly be regarded as the first pronounced physiological psychologist and his Latin '*Conjecturae*' as one of the earliest distinctive publications in the domain of physiological psychology.

IV

In tracing Hartley's development of the doctrine of association it still remains for us to determine the more precise relations on this subject of the Latin tract with the '*Observations on Man*.' The inquiry into the power of association in

respect to morality and religion and its physical bases began with the suggestion of Gay in 1731. Many other disquisitions on various topics followed. The Latin tract as we have seen appeared in 1746. His 'Observations on Man' was published in 1749, the preface, however, bearing the date of December 1748. In the three years which intervened between the two publications Hartley would obviously have "put together," as he says in the preface of the latter work, "all my separate papers on these subjects, digesting them in such order as they seemed naturally to suggest, and adding such things as were necessary to make the whole appear more complete and systematical." During this procedure it would be necessary to separate the Latin tract upon the doctrine of association from the medical treatise on Lithontriptics with which it had first appeared as a supplement. It is indeed possible that Hartley had also issued separate reprints of the Latin tract and that one of these, falling into the hands of Parr, thus ultimately appeared among his metaphysical tracts. In no other way can we so reasonably account for Parr's ignorance of its publication as an appendix to the medical treatise in 1746. What use then did Hartley make of the Latin tract in the preparation of the 'Observations on Man'?

The complete title of Hartley's chief work reads: 'Observations on Man, His Frame, His Duty and His Expectations.' The work is divided into two parts. Part I. contains his 'Observations on the frame of the human body and mind, and on their mutual connexions and influences.' Part II. is given up to his 'Observations on the duty and expectations of mankind.' Our attention is directed here solely to the first part, upon the 'Observations on the frame of the human body and mind, and on their mutual connexions and influences.' An examination of this discloses a distinct separation of his treatment into theory and application. In chapter one of Part I Hartley presents the doctrines of vibrations and associations in general. He seeks in it to 'lay down the general laws according to which the sensations and motions are performed and our ideas generated.' The subsequent chapters deal with the application of the doctrines of vibrations and

associations. In chapter two he inquires how far the general laws as already ascertained are illustrated by the particular phenomena of each of the sensations and motions; and in chapter three how far they are represented by the phenomena of ideas, or of understanding, memory, and imagination. In chapter four he investigates how far the pleasures and pains of imagination, ambition, self interest, sympathy, theopathy, and the moral sense are agreeable to the foregoing theory of vibrations and associations. And he concludes the entire discussion with a defence of the mechanism or necessity of human actions. But the general doctrines laid down in chapter one form the basis upon which the entire superstructure has been erected. It is necessary therefore in the pursuit of the early development of Hartley's doctrines of association to contrast the contents of the 'Conjecturae' with these general laws or theories of vibrations and associations as set forth at the outset of his 'Observations on Man.'

The contrast of the two works can the more readily be made inasmuch as Hartley regarded the form of mathematical demonstration as the most convenient to arrive at the force and meaning of things under association. Hence both in the Latin tract and in the 'Observations on Man' he proceeds by the geometrical method. In both works we find in their customary order propositions, corollaries, and scholia. Hartley also acknowledges alike in the introductory paragraphs of the Latin tract and in the 'Observations on Man' an indebtedness to Newton for the suggestion of the doctrine of vibrations, and to Locke and 'other ingenious persons' for the doctrine of association. He believes that one may expect that vibrations should infer associations as their effect, and association point to vibrations as its cause. The full purpose as expressed in the 'Observations on Man' is to explain the mutual relations of the doctrines of vibrations and associations. In so doing he resolved to pursue the method of philosophizing by analysis and synthesis recommended and followed by Isaac Newton.

Alike in the Latin tract and in the 'Observations on Man' Hartley presents a series of propositions which form the

complete framework for the development of his theories of vibrations and associations. In both treatises these propositions are twenty-two in number and follow the same order. Of even more significance, it appears that the twenty-two propositions of the Latin text have been translated without any variation of the contents in the English treatise. It can therefore be safely affirmed that Hartley merely reproduced in the propositions from the Latin tract the theories of vibrations and associations in the subsequent publication of the English text of the 'Observations on Man.'

With each proposition both in the Latin tract and in the 'Observations' there is given its evidence or explanation. A comparison of this evidence in the two texts is also illuminating. The evidence for almost every proposition is more fully given in the English than in the Latin text. Whereas in the Latin tract it is often stated where the evidence is to be found, there is given in the English version the reasons in detail which make up the proof. But while Hartley may enlarge or embellish the statement of the proof for the propositions in the English text, nowhere does he change the essential nature of the evidence existing in the Latin tract. If the reasons in the proof of the propositions have been numbered in the Latin tract they are similarly numbered in the English version, with additional numbers only in two instances. Wherever references are made to authorities in the Latin tract similar authorities are reproduced in the English text. Nowhere from beginning to end does Hartley depart from the nature of the proof of the earlier Latin tract when he presents the evidence for the doctrines of vibrations and associations in his 'Observations on Man.'

When corollaries are added to the proofs of the propositions in the Latin tract similar additions are also made in the 'Observations on Man.' In two instances only has their number been increased—in proposition XII from 4 to 9 and in proposition XIV from 5 to 12. What has previously been said of the resemblance as regards the direct evidence for the various propositions between the Latin and the English versions is in like manner true in regard to their corollaries.

These supplements to the proofs of the various propositions bear a corresponding agreement in both texts. The corollaries like the evidence, it is true, are developed with greater fullness in the English than in the Latin text. Nowhere, however, in the nature of the doctrines set forth in these corollaries is there any variation between the Latin tract and the 'Observations on Man.'

One special instance of resemblance in the doctrines of vibrations and associations between their Latin and English versions deserves here to be particularly mentioned. In the proof for the doctrine of vibrations alike in the Latin and the corresponding English texts an appeal is made to the nature of æther and structure of the nerves as set forth by Isaac Newton. It is thereby made to appear that external objects excite vibratory motions in the æther, these vibrations are transmitted to the medullary substance of the nerves and brain and as a consequence we have sensations. In like manner symbolic language of the Latin tract is reproduced in the English of the 'Observations on Man' for the support of the doctrine of associations. "Any sensations *A, B, C*," we read in both texts under proposition X, "by being associated with one another a sufficient number of times, get such a power over the corresponding ideas *a, b, c*, that any one of the sensations when impressed alone shall be able to excite in the mind the ideas of the rest." In the expansion of the proof of proposition X there is added, however, the statement that: "The influence of association over our ideas, opinions and affections is so great and obvious as scarce to have escaped the notice of any writer who has treated of these, though the word association, in the particular sense here affixed to it, was first brought into use by Mr. Locke." It is evident here that Hartley had made a careful historical study of the subject of association preparatory to the production of his great work in which he thus fully developed and applied his theory of association.

V.

It remains only as an outcome of what precedes to clear up the confusion of statements in regard to Hartley's position

on the question of necessity. Samuel Parr, it will be recalled, was quoted as saying in a letter to Dugald Stewart that Hartley openly declares in the Latin tract for the indifference of the will as maintained by Archbishop King rather than for the doctrine of necessity. Stewart, accepting as true Parr's statement that the Latin tract preceded in publication the 'Observations on Man' by only a year, naturally thought it curious that in such a short period Hartley's opinion upon so essential a point should have undergone such a complete change. In the three years which, contrary to Parr's opinion, we find to have intervened between the two publications, it is true there would have been more opportunity for a possible change of belief by Hartley. But since we have proved that the theories of vibrations and associations of Hartley's 'Observations on Man' are only an enlarged presentation of what is contained in his Latin tract and that in no essential doctrines do the two versions differ, it will readily be perceived how unlikely to be true is the statement of Parr of a change of view on Hartley's part in regard to the important doctrine of necessity.

At the close of the Latin tract Hartley adds a *scholium generale* in which he outlines the uses of the doctrines of vibrations and associations. The close relations of these doctrines to medicine, logic, and ethics are first mentioned in the *scholium generale* and are afterwards developed in the 'Observations on Man.' Although the *scholium generale* is not directly reproduced, it nevertheless served to suggest the extended applications of the doctrines of vibrations and associations found in this later English work. But in this development of his doctrines Hartley underwent no change of belief on the subject of freewill and necessity. The phrase *liberum arbitrium* does not even appear in the *scholium generale*. Indeed nothing is said in this *scholium* about freedom as necessity. As a matter of fact Parr is as much mistaken in the assertion of the presence of a doctrine of the indifference of the will in the Latin tract as we have found him to be in regard to the date of that treatise.

The actual growth of the doctrine of necessity in the mind

of Hartley is given in the preface to the 'Observations on Man.' "I think, however," he there writes, "that I cannot be called a system-maker, since I did not first form a system, and then suit the facts to it, but was carried on by a train of thoughts from one thing to another, frequently without any express design, or even any previous suspicion of the consequences that might arise. And this was most remarkably the case, in respect to the doctrine of necessity; for I was not at all aware that it followed from that of association, for several years after I had begun my inquiries; nor did I admit it at last without the greatest reluctance." We know also from Hartley's statement about Gay's suggestion concerning the power of association, that his own inquiries began in 1731, or eighteen years before the appearance of his 'Observations on Man.' But in 1738 we find him defending his own doctrine of determinism against the criticisms made by Lister over the rough drafts of a proposed 'Introduction to the History of Man.' Hence, with whatever hesitation Hartley had at this time accepted the doctrine of necessity, since he did not have a suspicion at first, as he says, that the doctrine of necessity would arise from the theory of association, his adoption of this view must have taken place in the latter part of the intervening period from 1731 to 1738. Reflection on the doctrine of association culminated in the Latin tract 'Conjecturae.' This theory was next reproduced with fullness in an English version which forms the first part of 'The Observations on Man.' In it the necessary association of our conceptions is found throughout to proceed according to immutable laws. And this agreement in association, absolutely necessary to our thinking, is regarded as a complete and decisive proof of the doctrine of necessity. Thus after years of attentive consideration Hartley was able to set forth the doctrine of necessity in a final form in 1749, in the second part of his classic work on 'The Observations on Man, His Frame, His Duty, and His Expectations.' Hartley's adoption of the doctrine of necessity was not therefore, as Stewart was led with much reluctance to believe from Parr's erroneous statement, the result of a sudden change of opinion,

but as Hartley himself affirms the outcome of a long period of serious reflection.

VI

The preceding discussion has thus shown that in opposition to various conflicting opinions of writers the first appearance of Hartley's Latin tract entitled '*Conjecturae quaedam, de sensu, motu et idearum generatione*' preceded the publication of his '*Observations on Man*' by a period of three years as it was attached to a medical work on Lithontriptics in 1746. Furthermore by an examination of the statement of the various propositions, of the evidence adduced in their support and of the corollaries which have been attached, it is learned that Hartley's doctrines of vibrations and associations were first presented by him in the Latin tract in 1746 and were merely reproduced by translation with extended proofs in his '*Observations on Man*' in 1749. Inasmuch as the doctrines of vibrations and associations held by Hartley appear in every vital feature in this earlier treatise, the origin of the association psychology must be traced to his Latin tract rather than to his well-known work on the '*Observations on Man*.' The year 1746, the date of Hartley's Latin tract, rather than 1749, that of his '*Observations on Man*,' is therefore the date of the birth of the association psychology.

THE COLOR RED, AND THE ANGER OF CATTLE

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In a larger plan for the study of anger in men and in animals, a study which is being assisted by a grant from the Research Funds of the University of California, it has seemed well to include experiments concerning the effect of red upon cattle. For it is popularly held that red arouses anger in them, and especially in bulls,—an effect which some would attribute to its being the color of blood. With regard to blood, which for the popular mind, and for some minds not merely popular, is, natively and apart from any experience of injury, a powerful emotional excitant of men and of cattle, I shall wish to report briefly in a later paper.

The evidence which follows, drawn from experiments and from the experience of cattlemen, will, I trust, help us to understand somewhat better the reaction to red itself and apart from blood.

Cattle were experimented upon by Miss Morrison, Research Assistant, Mr. Blodgett, Teaching Fellow,¹ and myself. And these cattle were of different wildness: from the tamest of dairy cattle; through less tame animals brought in from the range and still having considerable freedom and seclusion in the wooded and canyoned hills near Berkeley; out to true animals of the range remote and many miles in extent, and seeing few if any men but their own herders. These experiments with color were upon 40 head of cattle: upon a 5-year Ayrshire bull; upon a herd of 12 comprising 2 bullocks and 10 cows, all of them about 2 years of age; upon another herd of 12, consisting of 9 cows and 3 young calves; and upon a

¹ Besides these who so ably assisted me, I am indebted to Mr. M. E. Stroud, Foreman of the University of California Dairy, near Berkeley, and to Professor G. H. Hart, Associate Professor of Veterinary Science, for their hearty coöperation.

herd of 15 upon the range, of which 10 were 6-months steers, and 5 old and 'wild' cows.

The colors used were white, black, red, and green, in strips of cloth each measuring 2 x 6 feet. These were displayed, in all but one case which is noted, by attaching them endwise to a line stretched high enough to let the animals go easily under it; from this line the colors hung their 6 feet of length free of the ground, well-separated, and ready to flutter in the breeze. Sunshine made the red, green, and white especially brilliant. To our eyes the red and green seemed of nearly equal brilliance, the red being perhaps slightly brighter.

A few example-scenes will show the manner in which our brave banners were received.

The herd of 12 in which there were 2 bullocks, I urge gently toward the colors. At a distance of 40 or 50 feet the nearest cows stand at attention, the bulls remaining behind. One of the bulls then moves to the front, and there is a general slow movement forward with some gentle snorting. The other bull remains in the rear, feeding. Cautiously coming to the flags, there is much halting, with most attention to the *white*. Four cows sniff the *white*, and back slightly when it flutters towards them. Then one of the bulls smells of the *black* for a time; the other bull stands near the *red*, facing it, but with little or no sign of interest in it. One of them sniffs the *white*, glances at the *red*, and turns again to the *white*.

The 5-year bull when slowly driven along the line of colors now fastened along a high fence, showed perhaps a slight start at the *white*; and after passing this color he returned and sniffed it. Moving again past the colors, he gave at most a glance at the *red*, then stopped between the *green* and the *white*, and gazed up the hill. Again, he goes to the *black* and stops with perhaps a sniff at it, then turns and passes close to the *red* and stops with his rear to this color.

From repeated observations upon these and the other cattle, it would appear that:

1. There was no strong excitement from the colors as a whole or from any one of them. There was interest, hesita-

tion, mild mistrust toward all and any of the banners, as toward strange things.

2. None of the colors caused anything that could be recognized as anger.

3. Brightness and motion, rather than hue, were effective. There was no special reaction to red as against green, nor to red and green as against black and white. A slightly greater interest and mistrust was shown toward white than toward any other; and toward a fluttering banner than toward one at rest.

4. There was no clearly different reaction found in bulls, cows, steers, and calves.

Was this outcome due to the tameness of our cattle near Berkeley? That the bulls seemed no more excited by the colors than were the cows inclines me to answer 'no.' The experiments by Mr. Blodgett upon the range makes one the more confident in denial. The behavior of his reputedly wild cattle was like that of the cattle in the Berkeley Hills; he found interest, mistrust, a 'starting' when the banners were suddenly moved; but no wild excitement, no anger. He also found a larger response to the white.

It would seem well to add, then, that:

5. The reaction of wilder cattle to our colors was essentially like that of the tamer; they showed interest, curiosity, mildest fear, but no anger; and gave more response to brightness and motion than to hue.

It is evident that these observations from experiment are opposed to the popular idea that red excites cattle to anger. Have we also against us the united experience of those familiar with cattle in California?

Of 66 such persons who have favored me with their careful replies, I find that

38 believe that red never excites cattle to anger;

15 believe that red usually does not excite them to anger, although exceptionally it may;

8 believe that it usually so excites, though exceptionally it may not; and

3 believe that it always so excites.

Thus 53 of these experienced persons are opposed to the popular belief, while only 11 are supporters of it.

Yet the evidence of these believers is not to be despised; the following will illustrate what is perhaps the best they offer.

"A lively little Jersey cow whom I had known all her six years of life, chased me through a barbed wire fence when I was wearing a red dress and sweater, and never did so before or after. I changed to a dull gray, and reentered the corral, and she paid no attention to me, and let me feed and water her as usual. Also a Durham bull whom I had raised from a calf, and was a perfect family pet, chased me till I fell from sight through some brush when I was wearing the same outfit of crimson."

With this, might be put the observation of Hudson,¹ that a sullen cow could be brought repeatedly to charge a gaucho whenever he suddenly opened his poncho and displayed its bright red lining. Here, it should be noted, the animal was already angry and might well have charged even more readily, if the poncho's lining had been white.

The weight of judgment of our California cattlemen as we have seen, is against the common belief that anger in cattle is aroused by red. Some of them, however, venture what naturally now seems to me a discriminating opinion, that there is some exciting effect from *bright* colors generally. One of my informants says: "Cattle will not pay any attention even to a bright color unless they have been teased and made to go hungry for a long period. Sometimes range cattle are easily aroused, but that depends more on the way they are being handled than on the color they see." Another writes: "I have never noticed that red cloth would anger any of my stock. I have tried it and have seen that they pay no particular attention." Finally in referring to the saying, "Like waving a red rag before a bull," one of my correspondents writes: "I have found that to wave anything before a bull is dangerous business."

Our own observations are not only in accord with such opinions, but with much of Hudson's observation that red of

¹ *The Naturalist in La Plata*, 1903, 327-347.

itself and detached from human beings arouses *curiosity* only; although I should prefer to say that it arouses *curiosity tinged with fear*. Nor does it seem probable that he is wholly right in holding that red, when it is attached to a human being, becomes a challenge, an insult. To me it would seem probable that any unusual act or appearance,—a strange color, perhaps, or an uncommon brightness or a sudden unexpected movement—when connected with either a human being or an animal that upon other grounds is already an object of fear or anger,—that the unusual in this special connection intensifies, adds a spur to the present anger, or perhaps transforms the fear to anger. It thus may add the final touch to the tense state and throw it into action. If cattle distinguish hues, it may well be that a strange hue, say red, in a garment may add to the effect of strange movement or strange brightness. This is probably all there is of truth in the popular idea that red excites cattle and especially bulls to anger.

It is probable that this popular belief arises from the fact that cattle, and particularly bulls, have attacked persons displaying red, when the cause of the attack lay in the behavior of the person, in his strangeness, or in other factors apart from the color itself. The human knowledge that red is the color of blood, and that blood is, or seemingly should be, exciting, doubtless has added its own support to this fallacy.

DISCUSSION

A NOTE ON THE "EXPRESSION" OF SIMPLE FEELINGS

Among the vices of the critic none more certainly elicits the reproach of the author and of his friends than this vice of the misleading quotation,—misleading now by mutilation and again by omission of context. An alleged instance of the vice, under its first form, is cited by Professor Titchener in the January number of the *American Journal of Psychology* (1923, 34, 149). The offending quotation was made by Young out of Corwin's paper on 'The Involuntary Response to Pleasantness' (*ibid.*, 1921, 32, 563). "Though literally accurate and made, of course, in all good faith, [it] must nevertheless," as Titchener fears, "be misleading to a reader who has not the original paper very clearly in mind."

The charge of mutilation is always well designed to impress the compassionate reader, who finds it easier to suffer with the mutilated author than to hold "very clearly in mind" the sense of the argument. Were it not so, Young could rest assured that *his* readers would have observed that he had made perfectly clear in his context that Corwin had "proved the point," etc., under the prescribed conditions. Wanting that assurance, he might note that Titchener, while he condemns mutilation, exemplifies the selfsame vice under its second—its contextual—form; but that would be to borrow a defensive 'mechanism' which the brave soldier willingly employs only when life and honor are horribly menaced. Titchener should have been warned that Young's "therefore" (in the quotation "It is impossible, therefore, to accept Corwin's interpretation . . .") refers backward to the very qualification which Titchener seeks. In the next preceding paragraph (*ibid.*, 1922, 33, 522) Young states the conditions under which "'seeking' movements *may be* associated with P." It is possible that Titchener's disturbance lest he should have 'allowed' the alleged statement 'to appear in print' 'prepossessed' him (as he says of Young) and thus led to his oversight. An easy misstep from logic, and one 'made, of course, in all good faith'!

It would be regrettable if this slight rejoinder to Young's attack should distract attention from the main outcome of the studies under consideration. The first two papers, one by Young from

Minnesota and the other by Corwin from Cornell, implied the old error of regarding organic movements made during or subsequent to feeling as 'expressions' of the feeling. But it stands to the credit of Young, in the third of the articles (Illinois), to have suggested that the 'responsive' or 'expansive' movements, alleged for pleasantness under Corwin's conditions (and referred to by Titchener without reproof as the 'direct response of the organism to P' and as a 'definite activity of pursuit'),—that these movements had only a remote and adventitious connection with the affection. Again—a still more important matter!—Young here affirms that both 'pursuit' or 'seeking' and 'avoidance' are teleological interpretations on the level of behavior. Young sees and acknowledges the slip (which, as Titchener naïvely intimates, the Cornell Laboratory might also have considered had it 'been led into further discussion') and recommends that the 'traditional hedonistic doctrine be abandoned.'

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